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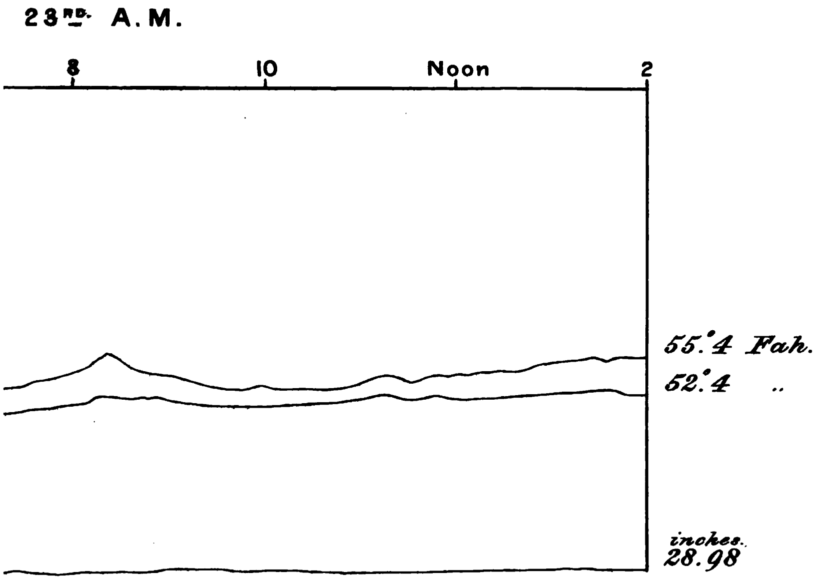

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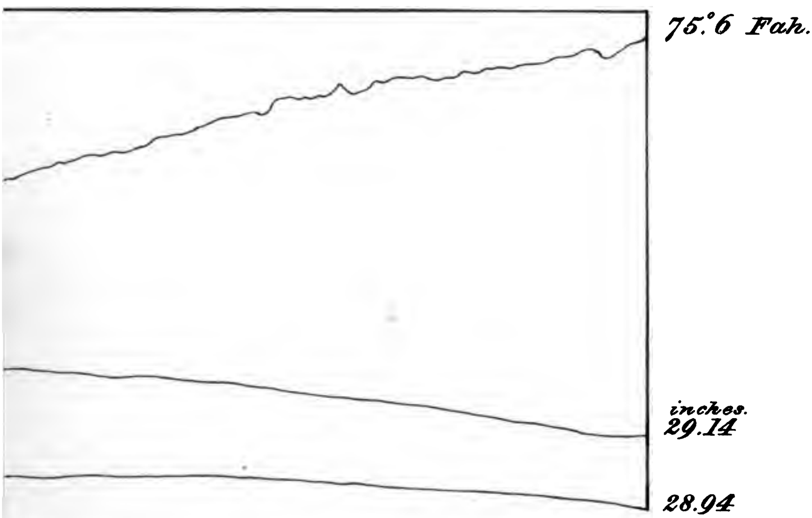

Fig. 5.

AN ESSAY

ON

THE CAUSE OF RAIN

AND ITS

ALLIED PHENOMENA,

BY

G. A. ROWELL,

HONORARY MEMBER OF THE ASHMOLEAN SOCIETY.

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CONTENTS.

APPENDIX.

ON THE CAUSE OF RAIN

AND

ITS ALLIED PHENOMENA.

Introductory Remarks.

1. OF all the ordinary phenomena of Nature, there are none which attract attention in so great a degree as those connected with rain, and the causes producing it. The most careless observer of natural objects must sometimes have his attention arrested by the glorious appearance of the clouds, when reflecting the rays of the setting sun, or by the more softened effects of an early dawn. The beautiful arrangement of cirrus or cirro-cumulus clouds in a "mackarel sky," or the magnificence of cumulus clouds, when piled like snow-capped mountains to an enormous height, cannot fail to excite the admiration of every observer; while the sublime and sometimes terrific phenomena of the thunderstorm will arouse feelings of awe in the most indifferent, and bring down the proudest among mortals to a feeling of his insignificance in the great system of Nature. And when we consider how important all meteorological changes are to man, as regards his health, sustenance and comfort, it is not surprising that all should feel interested in them. The first question with most persons daily is respecting the weather; and whenever any excess occurs, either of heat, cold, rain, hail or storm, the memory is racked to find some reminiscence of a similar state of things: every change of the weather is more or less noticed by all, and I believe that most persons have a vague idea of some means by

which such changes may be accounted for, or a lurking desire to obtain some such knowledge.

2. To those who make a study of meteorology, it is a never-failing source of mental enjoyment; as it is not only the most magnificent of its phenomena which is attractive —every phenomenon is so: the evaporation of a drop of water, the formation of a cloud, or the fall of a rain-drop, may be subjects of interest and for reflection; a deposit of hoar frost or rime affords some of the most beautiful objects for examination, and the microscope opens another source of delight in the brilliant crystals of snow.

3. The desire to acquire knowledge on these subjects is shown by the many persons who impose on themselves the task of registering the state of the barometer, thermometer, &c., and of noting down all unusual meteorological phenomena. These registers have led to important results, as regards the knowledge of barometrical fluctuations, and in affording data for ascertaining the mean temperature of places, the construction of isothermal lines, and a knowledge of the general distribution of heat over the earth. Observations on the hygrometer, the force of vapour, &c., have given like data as to the diffusion of vapour in the atmosphere, and other points of importance connected with meteorological investigations: but on the cause of spontaneous evaporation, the formation and suspension of clouds, and the general phenomena connected with the fall of rain, I believe that at present there is no established theory. Many theories have been advanced from time to time, yet scarcely any two works of a scientific character give the same, unless with some important variations.

4. I have no idea that the theory I propose will be fully adopted; still I hope it may prove a step in the right direction: at all events, I believe the ground is not preoccupied by any other which can or will be generally received; and therefore, as I have (although under unfavourable circumstances) devoted many years to the study of meteorological phenomena, I trust it may be found worthy of consideration. I may be thought rash in venturing

opinions on subjects which have engaged the attention of so many eminent men, or be charged with presumption in doing so; but I hope to be excused on this head, and in some degree secure the attention and sympathy of my readers, by simply stating the circumstances under which I have been led to bestow so much time and attention on these subjects, and to adopt the views I now seek to advance.

5. I cannot say at what time I first began to think about the causes of meteorological phenomena; but from my childhood one of my greatest delights was to watch the lightning and listen to the distant thunder. This desire grew upon me, and on summer evenings I generally contrived to get into some open place in hopes of seeing lightning; and there I have often watched a storm till it has approached so near and become so heavy that I have been too terrified to stay, and almost afraid to run home. While still a schoolboy, a neighbour, knowing my propensity, lent me a book which made the thunderstorm still more interesting. This book (Lovett's Philosophical Essays) was written to prove the fallacy of some of Newton's views on gravitation; it contained much that I could not understand, but it gave a very clear and detailed description of many electric experiments, and especially those on the electricity of the atmosphere, by Franklin, Nollet, Romas, &c.; and an account of the death of Professor Richmann by a stroke of electricity from an atmospheric electric apparatus. For a time my attention was almost entirely devoted to this book, and those parts on electricity I learned almost by rote.

6. Bringing the knowledge I had thus acquired to bear on the phenomena of the storm, I was first puzzled to account for the electric state of the cloud: from the experiments given it appeared that when a pith ball or any similar object was brought into contact with a charged conductor, it acquired a like electric condition, and no further exchange of electricity could take place between them, till some change had occurred in the electric state of one or the other of those bodies; while the particles of

the cloud which had arisen from the earth had, somehow or other, become so electrified as to give off electricity to the earth from which they had risen.

7. I endeavoured to find an explanation of this point, by reading all I could meet with bearing on the subject; but so far from obtaining a satisfactory solution of the question, I was still further puzzled to account for the ascent of vapour, and the suspension of clouds in the air; water being so much heavier than the medium in which it was suspended. From that time, to theorise on the cause of rain and its allied phenomena, became with me a ruling passion; it has been the leading subject of my thoughts by day, of my dreams by night, and during several periods of ill health, when incapable of physical exertion, it has afforded me a never-failing source of mental recreation. In fact, so much has my mind, at times, been engrossed by these subjects, that I have often determined, during the harass of business, to think no more about them; but the first flash of lightning, or peculiarity in the weather, has brought them back to my mind as vividly as ever, and my thoughts have been occupied as before.

8. The theory I now hold occurred to me while still young; and from that time, every meteorological phenomenon I have seen or read of, I have endeavoured to account for on that theory, and all have seemed to me fairly explicable by it.

9. One of the first ideas which occurred to me in reference to the theory was, that it would be possible to cause rain; but I felt assured that by giving publicity to such an opinion, I should only subject myself to ridicule; and although fully convinced on the point, the fear of being laughed at kept me tongue-tied on the subject, not only when a boy, but for years afterwards.

10. I was upwards of thirty years of age when I attended a lecture on Light by the Rev. Baden Powell; at the conclusion of which, after speaking on the benefits arising from mental recreation and scientific pursuits to persons of all ranks of society, the Lecturer stated his opinion that the observations and researches of many per-

sons, which might have led to useful results, were often lost to the world, from diffidence in the individual or the want of encouragement or advice; and expressed his willingness to forward the views of any person, however humble, who was interested in scientific pursuits, or had any useful object in view of a scientific character. I felt at the time as if personally spoken to, that every word was addressed to me alone, and at once determined to call on the Rev. Professor and explain the theory and opinions I had formed; yet, still afraid that my proposition for causing rain would be looked on as ridiculous, or something worse, it was some months before I could summon up courage enough to call; but on doing so I was kindly received, my statement was listened to with patience, and I was then advised to commit my ideas to writing, and submit them in that form for further consideration.

11. Up to that time I had never written a note on the subject; and being unused to composition on that or any other subject, it was some months before I commenced my task: however, one evening I did commence, and after writing a few paragraphs I felt so assured that my views were reasonable, and became so interested in my subject, that I did not cease writing till I had completed my statement. I left it the next morning for Mr. Powell's perusal, who returned it to me, and, to my great surprise, with a note, advising me to send it to the London and Edinburgh Philosophical Magazine for insertion. I had never contemplated giving publicity to my opinions, and it was with some hesitation I adopted that course. It did not appear in the Journal, but, still favoured by that gentleman, it was read at a meeting of the Ashmolean Society, and at a meeting of the British Association at Glasgow in 1840; after which (1841) I published it in a pamphlet, together with another paper on the Aurora and Magnetism[a].

12. I was now enabled to pursue the study of the phe-

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[a] *Conjectures on the Cause of Rain, Storms, the Aurora and Magnetism: with a Suggestion for causing rain at will.*—Cambridge, 1841.

nomena of rain, and more especially of the aurora and magnetism, under very favourable circumstances in comparison with my previous position; the use of the library of the Ashmolean Society being afforded me, and many Members of the Society offering the use of such of their own books as I wished to consult. I have not been able to avail myself of these advantages so fully as I could have wished, or devote so much time of late to these subjects as I would gladly have done; but by the aid thus afforded I have acquired a knowledge of many facts bearing on the subject of this Essay, which I hope will enable me to set my opinions forth in a much clearer manner than heretofore. But with all these advantages I have seen nothing to cause me to deviate in the least degree from the theory I adopted many years since, and which was given in my first pamphlet on these subjects; on the contrary, every new fact which has come to my knowledge has tended to increase my conviction of the correctness of my early opinions.

13. It is now nineteen years since my first paper was read, and since that time several others have been submitted to the Ashmolean Society, in the reception of which I had good cause for satisfaction. At the meeting of the British Association in Oxford, in 1847, I read two lengthy papers fully explaining my views: on that occasion the section was well attended; I was listened to with attention, and afterwards congratulated on their reception, by several gentlemen who till that time were strangers to me. I may further add, that every paper I sent to the Edinburgh New Philosophical Journal was published in that periodical, some of them being copied into others. Several of my papers I have also had printed for private distribution, and copies have been forwarded to very many persons who seemed to take a leading part in meteorological investigations.

14. Thus the theory I propose, and the opinions I seek to advance, have been before the public in various forms for many years; but I am not aware that they have ever been controverted, except on one point of minor import-

ance, and even on that I was enabled, within a few months, to prove that my opinion was correct. I am well aware that all this does not amount to an admission of the theory, but believe this statement will show that I am not rashly endeavouring to obtain for my opinions a fuller consideration than has hitherto been bestowed on them, and that I am not very blamable, if I have been led to think them worthy of more notice than they really deserve.

15. As I am well aware that I am deficient in many qualifications for writing on a question embracing so wide a field as that of the Cause of Rain, it may be necessary, for the information of those of my readers to whom I am unknown, to state the circumstances under which my investigations of these subjects were carried on. Forty years since the education of youth was in general very inferior to that of the present day; mine was only that of (at that time) a common school, the teaching in which extended no further than to ordinary arithmetic, writing, and reading; and from this I was taken, when under ten years of age, to attend on an aged grandfather. From that time I had no chance of improvement, except from self-education, and even for that my resources were very limited. I may therefore say with truth, that my books were of Nature's printing, and the storm and its phenomena my field for study. Of the higher branches of arithmetic I know nothing, or of any language but my own; I have therefore none of the advantages which a knowledge of mathematics confers, and all foreign books are in a measure sealed to me.

16. I make this statement in deprecation of extreme criticism, as the arguments in my Essay may be improperly put, the style may be defective, many important facts may be unnoticed which a writer on the subject ought to take into consideration, and I may have misconstrued the writings of others. On these points, therefore, I claim some forbearance; but with respect to the theory I propose I ask no favour; that must stand or fall on its merits: my only request is, that it may be judged of in all its bearings.

17. It may be necessary for me to offer a few remarks as to the originality of my opinions, as some time after the publication of my first pamphlet it was pointed out to me that in the early part of the present century captain John Williams had proposed a similar plan for causing rain[b]; and since then I have been informed that the theory on the cause of evaporation, &c. is, in many respects, similar to one advanced by Henry Eeles, esq. in 1753, and published in the Transactions of the Royal Society[c]. It may be found that none of what I advance is new, but on this head I care nothing. It is certain that if such a theory was proposed it was not adopted, and all I have to say on the subject is, that whatever opinions I put forward as my own have been borrowed from no one; they are those I adopted when very young, and at a time when I had no chance of seeing the works mentioned; and I can assert, that in writing my various papers I have consulted no book or person except so far as regards facts[d].

On the insufficiency of existing theories.

18. Although no theory on the cause of evaporation, rain, &c. has been generally adopted, several are given in our Encyclopædias and works on meteorology; and as readers often adopt the views given in such works (sometimes without thinking on the subject themselves), it is necessary for me to show the insufficiency of those theories, to obtain a fair consideration of my own. A great objection to one and all of them is the want of simplicity. I

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[b] *Climate of Great Britain.* London, 1806.—This gentleman's proposition went further than mine, as it was not only for producing rain when wanted, but also for preventing rain when not required.

[c] See *Philosophical Transactions*, vol. xlvii. p. 524, and vol. xlix. p. 124.—The theories, as far as regards evaporation, are very similar, but after that there is little resemblance.

[d] To my friend Mr. W. Luff, of the Cornmarket, I am indebted for several calculations, which have greatly assisted me in many respects.

know of no published theory on evaporation in any such works which, even if it does not fail on some important point in reference to that subject, will account for the formation and suspension of clouds, the cause of rain, the electricity of clouds and other allied phenomena, without the aid of some other separate and distinct hypothesis as the phenomena vary; and none give that clear and ready explanation which may be fairly expected if a theory be correct. But it may be well to go more into detail on some important objections, which seem to me fatal to the theories to which I allude[e].

19. The theory most generally adopted is, that evaporation is caused by the particles of water absorbing heat, which becoming latent converts them into vapour, and their specific gravity being thus reduced they are rendered buoyant in air.

20. That heat is thus absorbed during evaporation is certain, and so also that, all other circumstances being alike, evaporation is accelerated by an increase of temperature: but that heat is the only, or even the principle cause of evaporation at atmospheric temperatures, is questionable on many points.

21. On this and the following theories it is assumed that water, in the process of evaporation, undergoes some peculiar change (becoming gaseous), and mention is made of visible and invisible vapour as differing in some important points. Water, when converted into steam at high temperatures, may undergo some important change; but that it does so during evaporation at moderate temperatures is assumed without proof, and it is probable that vapour, when invisible, is so from its being so diffused. But I will waive this part of the question for the present (151), and proceed to the consideration of other points more immediately bearing on the theory in question.

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[e] I here beg, once for all, that I may be excused if I seem presumptuous in advancing my own opinions, or dogmatical in reference to others; my object is to have the theory fairly canvassed; and this I hope to attian by throwing it fairly open to criticism.

22. One fact that tells against the above theory is, that evaporation will go on with considerable rapidity from ice, even when the temperature of the surrounding air is many degrees below that of freezing: this is shown by the rapid disappearance of snow during severe frosts, and has been proved by experiments. Newly plastered walls will become dry in sharp frosty weather, and even wet cloths, if hung out under similar conditions, will, as is commonly said, freeze quite dry.

23. "Experiments on the evaporation of snow are by no means numerous. Howard mentions a case wherein the vapour from a circular area of snow of five inches diameter, amounted to 150 grains from sunset to sunrise in January, and 50 grains more by the following evening, the gauge being exposed to a sharp breeze on the housetop. An acre of snow, under such circumstances, would have evaporated, in the course of 24 hours, the enormous quantity of nearly 64,000,000 grains of moisture. Taking, only the portion evaporated during the night, a thousand gallons were raised from the surface referred to. We may hence see why a moderate fall of snow sometimes entirely disappears during a northerly gale, without the least sign of liquefaction from the surface."

"Gay Lussac proved the evaporation of ice to go on at two degrees below the zero of Fahrenheit's scale."—From the *Encyclopedia Metropolitana*, article Meteorology.

"Mr. Holdsworth tried several experiments on evaporation from ice at Lake Winipeg, 52 north latitude. He suspended, under a shed, a piece of ice about two inches thick and 20lbs weight, from November 28 to February 14, when it had lost 17 ounces; the thermometer had not, during that time, been less than 9° below the freezing point of Fahrenheit."

24. It is quite unnecessary, as far as regards the generality of my readers, for me to offer any proof of the absorption of latent heat by ice in melting, but it may not be so to all. I therefore give the following, thinking it best to err on the right side.

25 It was proved by Dr. Black that 140° of heat are

lost, or become latent, during the melting of ice. This was shown by a variety of experiments, but a description of one will suffice. If any quantity of water, say 1lb, at the boiling temperature, 212°, be mixed with an equal quantity of water at the freezing temperature, 32°, the result will be 2lbs at the medium temperature 122°. But if a pound of water at 212° be mixed with a pound of snow or crushed ice, as near as may be of the temperature of 32°, 140 degrees of heat will be lost, absorbed, or become latent in the ice in melting, and the result will be 2lbs of water at the temperature of 52°, instead of 122°, as in the first case. Thus water is ice which has absorbed 140° of heat, or ice is water which has lost that quantity.

26. Now as ice requires 140 degrees of heat to convert it into water, it is difficult to conceive how particles of ice, when surrounded by a freezing atmosphere, can absorb enough heat, not only to convert them into water, but to make them upwards of 800 times lighter than that fluid, which they must be to render them buoyant in air, even in its lower stratum.

27. Again, the great height at which clouds have been seen tells against the theory. Clouds were seen far over head by Humbolt when at the greatest height he attained on Chimborazo; and also by Gay Lussac from his greatest elevation during his celebrated balloon ascent. These clouds could not have been less than 5 miles above the level of the sea; and being *visible* vapour, must, on my theory, have been condensed into water. Now as water at that height is about 2700 times heavier than the air (116), and the temperature of the air some 20 or 25 degrees below zero, it seems obvious that the ascent of vapour, and its suspension at such height, cannot be accounted for on the hypothesis of the absorption of latent heat.

28. It may be objected that 5 miles is an extreme height for clouds; it is so, but as in a great degree they would be invisible from moderate heights, we have no proof that they do not often attain that elevation; *and a theory, if correct, should meet all cases.* However, take 3

miles, which is not an extraordinary height, and the objection is still, I believe, fatal to the theory, as there the air is 1716 times lighter than water, and its temperature as low as 9° of Fahrenheit; while the expansion of steam from boiling water is not more thon 1800 times.

29. In fact, the suspension of clouds is difficult to account for on any of the theories under consideration; for allowing that water, by the absorption of latent heat or any other cause, is carried up in the state of *invisible* vapour, and is in that condition buoyant, even at great heights, it could not remain suspended when condensed into visible vapour; there must then be some agent to support it, which is in a measure independent of heat or cold, as it is certain that the higher the clouds, and consequently the colder and rarer the medium in which they are suspended, the less is the chance of their producing rain. And we often have clouds in very severe frosts without snow or rain.

30. Another theory given in a popular treatise on the subject is, that the molecules of water when at the surface and released from the influence of those below, after absorbing the necessary latent heat, acquire a state of polarity, and thus becoming self-repulsive, fly off into space in the form of vapour, diffusing themselves "in the same manner that one gaseous body is diffused through another."

31. The evaporation from ice, and the suspension of clouds at great heights, tell against this theory in like manner as against the preceding, to which it is very similar, while the change of vapour from the invisible to the visible state, and its aggregation into clouds, is more difficult to account for on this than the foregoing theory; for so long as the particles of vapour retain their state of polarity, and remain self-repulsive, they could not form clouds; and if they change from that condition, they could not remain suspended in air.

32. Evaporation has been explained as a gradual solution of water in air, as sugar or salt is taken up and held in solution in water: and rain as similar to the precipita-

tion of those solids when the solution is too strong or its temperature is reduced. This comparison hardly holds good, as sugar, salt, and such soluble substances differ but little in weight from water, while the difference in the specific gravity of that fluid and the atmosphere in its densest parts is about 860 times. The theory, however, has had powerful advocates, but the fact, that evaporation goes on with the greatest rapidity in vacuo, seems to be a direct contradiction to it; the heavy rains which sometimes occur cannot be accounted for as a precipitation from such a solution: and the theory fails in accounting for the suspension of clouds at great heights, and other phenomena connected with the subject.

33. Another theory, which has been supported by many eminent men, and which still has its advocates, is, that water in evaporating, takes the form of hollow spherules filled with very highly rarified air, and they thus become buoyant in the atmosphere. In this theory much is assumed and hypothetical: of the formation of these vesicles we have no proof; and if it were possible that air could be so rarified as to be buoyant when thus enveloped in so heavy a capsnle, it must be condensed on rising into the air, and the vesicles could not then retain their form and remain suspended.

34. Again, microscopes will magnify to full a thousand diameters; therefore an atom of water cannot have a diameter of the 200,000dth of an inch, or it would be visible (72); now as a particle of water must be expanded to upwards of 800 times its bulk to float in the lower parts of the atmosphere, and twice or thrice as much to float at the heights where clouds are seen, it is difficult to conceive how vesicles can be found of such extreme tenuity; or if so, how they could retain that form under the pressure of the denser air.

35. It is true, that we have the high authority of Saussure in support of this theory, who states, "that in one of his Alpine journeys he saw vesicles float slowly before him having greater diameters than peas, and whose coatings were inconceivably thin." I am not aware that any

other person has ever observed a like phenomenon, and with all due respect to that great man's memory, I think he may have been deceived by appearances. When the eye is more than usually humid, such an appearance of vesicles may at any time be produced by looking at the flame of a candle or other bright object with the eye nearly closed; and such an effect may have been produced by the glare of the sun's rays reflected from the snow: the coldness of the air may have added to the effect, and if at a time when the mind was otherwise occupied, it may have passed without further consideration, especially if the phenomenon was in accordance with a pre-adopted theory; for I believe all persons are too apt to be satisfied with first impressions if they coincide with their views[f].

36. A theory which at one time had its supporters is, that the vapour of the atmosphere is owing to the ascent of hydrogen and oxygen gases to great heights, where, by the agency of electricity, they are converted into water,

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[f] A curious circumstance once convinced me of the readiness with which we may be deceived by appearances. I was one night returning from the country, when I picked up a glow-worm; and thinking my warm hand would injure it, I placed it on my hat, intending to take it off again before I entered Oxford. After a while I took off my hat, and looking carelessly around it missed the glow-worm, and supposing it had fallen off, I thought no more about it. Some time after this, I was suddenly surprised by the appearance of an extraordinary meteor, which, as I raised my head to look at it, passed with great velocity right across the zenith, in a straight line as far as I could see it by throwing my head backwards; but when there, the course of the meteor became very eccentric, turning about in every direction as I moved my head to look at it; and then, but not till then, I discovered that it was my glow-worm's tail just protruded over the edge of my hat above my eyes. But so completely was I deceived, that had the light been withdrawn before it had left the straight line of its path, I should have believed that I had seen a most extraordinary meteor, larger than the moon, and far brighter.

and fall as rain. On this theory I need only observe, that its fallacy is shown by the now known laws of the diffusion of mixed gases, and the suspension of clouds is altogether inexplicable on it; in fact, I believe it is now so little thought of, that I should not have alluded to it, but that not long since I heard a public lecturer, after exhibiting the formation of water by the explosion of the mixed gases, describe it as an illustration and confirmation of that theory.

37. Having briefly stated the various theories on evaporation, and shown some objections which seem to tell against them, it would be tedious to go over all the points connected with the subject in reference to each theory; but I believe that if fairly considered with regard to the most striking of the phenomena and the causes producing them, if the theories are not contradicted by facts, they will fail in affording a satisfactory explanation.

38. To the question of the suspension of clouds I have already alluded, but there is one theory, put forward in a popular and interesting treatise, on which it may be well to make a few remarks. To account for the suspension of vapour, after its change from an invisible to a visible condition, it says, "The only explanation yet given which is consistent with the known facts is, that the watery particles are suspended by their adhesion to particles of air, just as water is suspended from any solid body. This force upholds the watery particles when they are bereft of the self-sustaining power of elasticity or mutual repulsion."

39. That minute particles of water are kept up, or rather retarded in their downward course, by the pressure of the air, is not only probable but certain; but that these particles, which are from one to upwards of two thousand times heavier than the medium in which they float, can really be held up by adhesion to particles of that medium, is not only doubtful but seemingly impossible.

40. The theory generally given on the cause of the precipitation of rain is Dr. Hutton's, which is, that as the amount of vapour or moisture which can be suspended in

a given quantity of air increases as the temperature of the air is increased, but in a greater degree, therefore two bodies of air, when *saturated* with vapour, but of unequal temperatures, say 50° and 70°, would hold more vapour in suspension than could be suspended in the same air if mixed and at the medium temperature, 60 degrees; consequently, there must be a surplus of vapour which would be precipitated as rain. On these grounds it is supposed that rains are the result of the mixture of such bodies of air, and consequent precipitation of surplus moisture.

41. This theory is attractive from its ingenuity, and may be considered as sufficient to account for moderate rains, but it totally fails when applied to such rains as are noticed hereafter; and the theory which fails in explaining the cause of rain in one case cannot be adopted with propriety in another.

42. Again, the meteorological registrations of the storm of September 22, 1856 (hereafter described), which commenced near Glastonbury, and passed by Oxford (nearly over the Radcliffe Observatory), tell strongly against the theory; for although the heavy clouds almost swept the ground, and during the five minutes the storm was passing the rain was as much as the tenth of an inch, yet the thermometer does not appear to have been in any degree affected by the storm or heavy dash of rain.

43. The electricity of the atmosphere has been attributed to various causes, but none seem to have been satisfactory; and in some meteorological works the question is in a great degree avoided. Friction of the air against the earth has been assigned as a cause; and also the meeting and friction of different currents of air. The latter opinion having been most generally adopted, but purely, I believe, on hypothetical grounds; as I am not aware of any means by which electricity can be developed by currents of air artificially produced, and the experiments of Beccaria, both on this point and on the effect of winds in *lessening* electric intensity, tell directly against that opinion.

44. In all works on the subject the electricity of the

atmosphere seems to be considered as a matter of minor or secondary importance in the production of meteorological phenomena, and almost as if arising from accidental circumstances, instead of being (as I hope to show) the principal agent in producing them. The fluctuation in the barometer in calm weather, previous to or during rain, is in no way explained that I am aware of: and the formation of such large hailstones, or rather pieces of ice, as sometimes fall during heavy storms, is not fairly accounted for. But these questions will be discussed hereafter.

On the Materiality of Electricity.

45. In the foregoing chapter I have objected to the different theories on the ground of their want of simplicity, and also their insufficiency in explaining the various phenomena connected with the subject without the aid of some other separate and distinct hypotheses; and have also given as a rule, *that a theory, if correct, should meet all cases.* I of course expect that my views will be tested by the same standard, and believe the theory I propose has the merit of simplicity; and so far from requiring one hypothesis for evaporation, another for rain, and so on, I hope to show that the phenomena of evaporation, the suspension of clouds, the cause of rain, hail, lightning, and the *local* winds of temperate regions, may be fairly explained by it.

46. *The theory is, That the atoms of water being so minute are, when completely enveloped in their natural coatings of electricity, rendered so buoyant as to be liable, even when in their most condensed state, to be carried off by slight currents of air; but if expanded by heat, their capacity for electricity being increased by their increase of surface, they are then rendered buoyant at all times, and are buoyed up into the air by their coatings of electricity; when, if condensed, they become positively electrified, but are still buoyed up by the electricity, till, on the escape of the surcharge, the particles fall as rain.*

47. This is the theory in brief; it is dependent on cer-

tain properties which have been generally attributed to electricity; i. e. that it occupies space, has no weight, and diffuses itself over the surface of bodies. But as these properties are now disputed by many, and few persons fully agree on all points as to the nature of that agent, it is necessary for me to state, at some length, what I believe its properties to be; as otherwise there will be but little chance of my being understood.

48. I am fully aware that I am entering on this part of my subject under great disadvantage. I have been unable to investigate the abstruse points on which discussions on the subject now seem to turn, and the more minute and difficult experiments connected therewith have been beyond my reach; the few I have tried being of the most simple character. If therefore I run aground with my arguments, I trust the reader will set my bark afloat again, rather than swamp it at this point; or in other words, will endeavour to see the end I aim at, if I do not treat the subject so clearly as is desirable.

49. The theory of electricity which is taken as the foundation of the theory I propose is (I believe the Franklinian), that it is a material fluid (or anything else we please to call it), which is impelled with intense force to distribute itself equally over the surface and in the pores of all bodies (not gases), and that all electric phenomena, be it attraction, repulsion, or induction, are attributable to this property of electricity.

50. By negative or positive I would be understood as meaning minus or plus its natural condition, and not as distinctive of two kinds of electricity. Yet a body may be in a positive state of electricity, as regards the natural or normal electric state of the earth, but negative with respect to any other body more highly charged than itself. A body may thus be more and more positive as its charge of electricity is increased; but by the term *surcharged,* I would in all cases imply a charge in excess of the general electric condition of the earth.

51. One of the leading properties of electricity is, that no body can have more or less than its natural share of

that fluid, unless the body so circumstanced be kept in that condition by some force; and when such is the case, the electricity is intensely impelled to restore the equilibrium which is thus disturbed.

52. If the equal distribution of the electric fluid be undisturbed, no electrical phenomenon is exhibited; but if from the effects of friction, expansion or contraction of surface, or any other cause, a body *A* acquires more, or loses a portion of its natural charge of electricity, then it attracts and is attracted more or less by all other dissimilarly electrified bodies surrounding it, from the impulsive force of the electric fluid, on either side, to regain its equilibrium; the energy of the attraction being in proportion to the difference in the electric condition of the various bodies.

53. The *apparent repulsion* of bodies similarly electrified, either positively or negatively, may be explained as owing to a like cause. Thus if a body *A* were free to move, it would place itself in the centre of the forces acting on it; then if another body *B*, in the same electric state, be brought near to *A*, these bodies, being equally electrified, would have no electric attraction for each other, but the force acting on *A* would be cut off on the one side by the approach of *B*, and *A* would be drawn away by the forces still acting in the opposite direction, and not from its being repelled by the electricity of *B*.

54. Or thus, if a globe be either *negatively* or *positively* electrified, it will attract and be attracted, more or less, in every direction; now if the globe be so fragile, as that this attraction is sufficient to separate it into minute fragments, these, having no attraction for each other, would be attracted apart by the influence of surrounding objects, and not dispersed through any repulsive force amongst themselves.

55. I am anxious to be understood on this point, as I have no idea of any repulsive property in electrified bodies *beyond the extent of their electric coatings when surcharged.*

56. The phenomena of induction I believe to be thus:

if a positive charge be given to any body, and more than a natural amount of electricity be thus brought within a certain space, all surfaces in the neighbourhood of the charged body would have their natural attraction for electricity reduced; this effect lessening as the distance from the disturbing cause is increased; consequently the electricity would recede more or less from these surfaces, and a negative condition be thus induced. Or if the body be brought into a negative condition, there would then be a minus quantity within the same space, and electricity would be impelled towards that point, and restore the equilibrium; but if this were prevented by the intervention of a non-conducting body, then a positive condition would be induced in the neighbouring bodies.

57. That electricity is material, and occupies space, is disputed by many, including some of the leading electricians of the day; yet many, with myself, do hold that opinion, and (although with some diffidence) I will endeavour to show that I have some grounds for doing so, the truth of which is in fact the ground of the theory I propose.

58. By those who hold the former opinion it is contended that electricity is only a force. I confess that I really do not fully comprehend what is meant by a force; I will therefore, in addition to what I have already stated, endeavour to explain what I mean by being material. If to exist and to be here, there, or somewhere, and at all times to occupy space, is to be material, then I hope to show that there are fair grounds for believing that electricity has these properties: but if it is necessary that all matter must be ponderable, and subject to the ordinary laws of gravitation, then electricity may not be material.

59. That it is necessary for the purposes of nature that gases and all ponderable matter should gravitate towards the centre of the earth is obvious; but if electricity had been ponderous in that sense, I believe it could not have been the important agent it now is in the operations of nature; its effects seeming to depend on its tendency to gravitate (if I may use the term) to the surface of all bodies, and the intensity with which it is impelled to

the restoration of any disturbance of its equal distribution.

60. One ground for assuming that electricity occupies space is, that a body may be charged with electricity under the pressure of the atmosphere, but no charge of any consequence can be retained on any body in vacuo, or in highly rarified air: this seems to show that electricity is sufficiently gross to be pressed on and restrained by the pressure of the atmosphere, and consequently, that electricity occupies space.

61. The effects of lightning, or the electric spark, seem to show that something passes; a certain degree of intensity being obviously necessary to overcome the resistance of the air, therefore there must be something to be resisted. The effects of electricity in motion tends to prove the same, as its passage is free and rapid through conductors, if of sufficient capacity; but if too small, they are heated, fused, or dissipated, while non-conductors are invariably more or less damaged. And when we see the destructive effects of lightning, it is difficult to conceive how such can be produced unless by the passage of some material agent.

62. It is admitted (I believe generally) that the electric spark is the effect of the compression of the air, from the rapid passage of electricity through it; the intensity of the light being in accordance with the density of the air in which it is produced. This may be advanced as another proof of the materiality of the electric fluid, as it is seemingly inexplicable on any other supposition than that there must be something material to produce the compression. The same argument holds good respecting the currents of air produced by the passage of electricity from pointed conductors, &c.

63. From all I can learn on the subject, those who deny the materiality of electricity attribute all electric phenomena to a change in the condition of the molecules of matter, or a sort of polarity induced in the particles of the bodies affected; so that the passage of electricity (as it is generally termed) is not in fact the passage of any-

thing, but rather a current of changing conditions: therefore to electrify a body is simply to change the polarity of its particles, and consequently, electricity is not a material element, it occupies no space, and the weight of a body cannot be affected by its being electrified.

64. The foregoing opinion is, I believe, chiefly supported on the following grounds; i. e. that electricity cannot be proved to occupy space; that no difference can be detected in the weight of a body when charged with electricity or not; that its passage through metals and other dense conductors tells against its being material; and that its intense velocity can be more readily accounted for on the supposition of a change in the polar condition, or vibration, of the molecules of matter, rather than as the passing of a material fluid. There may be other important points connected with the question, but these, I believe, are the principal, and on them I will venture a few observations.

65. That electricity cannot be proved to occupy space, is no proof that it does not; and to give much weight to that argument, it is necessary to show that the question can be tested by the means adopted for the purpose. I believe that no satisfactory test can be applied, and that such an amount of electricity cannot be insulated as would suffice to settle the question. For although the effect of a discharge of electricity may be great, the agent producing it may be restricted to a small space, and there are (I believe) good grounds for this opinion. The light from a flash of lightning, or an electric spark, may be brilliant and dazzling, but these are effects, we do not see the agent producing them. The charge from a large battery may be passed through a card, but the perforation will be very minute.

66. A heavy discharge of lightning may be carried off harmlessly by a small but good conductor. A bell wire has done so for many yards, where the lightning has been destructive both before and after taking to that conductor. During the investigation directed by the Board of Admiralty as to the effects of lightning conductors, previous to

adopting the plan proposed by sir William Snow Harris for the protection of ships from lightning, it was stated that "a copper rod of half an inch diameter had never been known to be fused or heated red hot by an atmospheric discharge of electricity;" and there can be but little doubt that a conductor of twice that diameter might be carried through a powder magazine, and the heaviest lightning stroke passed off harmlessly by it.

67. A short time since a farm house near the Woodstock-road station was struck by lightning; the effects were singular, one of them being the perforation of a bandbox, of near a foot in depth, containing a silk dress, and other articles. There were holes in the wall showing the effects of the lightning, but the holes where it entered and passed out of the box were so small that they escaped notice, till on the dress being taken out a few days after the occurrence, it was found to have been perforated through all its folds, as if a hot knitting-needle had been passed through the box and its contents.

68. These cases, and others which might be adduced, show that however destructive electricity in motion may be, its real sphere of action may be within a very small space, or rather that the agent producing such effects may be small in magnitude; and therefore inappreciable to weight or measure.

69. To this view it may be objected, that if electricity be material, and yet occupies so little space as to admit of no admeasurement, it must be insufficient to produce the effects I attribute to it as the principal agent in meteorological phenomena. But I will endeavour to show that a coating of electricity, far too slight to be really measured, would be quite sufficient for those effects, if it possesses the properties attributed to it on this theory.

70. I have already (34) taken the 200,000dth of an inch as the extreme diameter of an atom of water: now although water is 860 times heavier than air (116), a particle of that fluid would be rendered freely buoyant by an envelope of imponderable matter of $4\frac{1}{2}$ its diameter in thickness, as that would increase its bulk 1000 times; conse-

quently there would be a buoyant power of 1000 to 860 the gravity of the atom of water.

71. It is by no means certain that the *surcharge* of an electrified body is ever equal to its usual coating; i.e. that a body when surcharged can have double its natural quantity, and such a surcharge would far exceed the requirements for this theory: but even if it could be so, I cannot conceive how the effect of such an extremely thin coating could be measured. The attempt to ascertain the space occupied by the electricity must be made under the pressure of an atmosphere, as it could only be known by the amount of air displaced by it, and the slightest change of temperature might affect the pressure of the air, and more than counterbalance the effects of the minute quantity to be measured. Again, the electrified body must have at least a surface of conducting material; it would therefore be impossible to ascertain in what degree the electric charge might enter the pores of the conductor, which pores may be inaccessible to particles of air, and thus the electric matter might occupy space, without displacing the air pressing on the charged surface.

72. And even if these objections have no weight, still it seems impossible that the presence of such a mere film could be ascertained, or any displacement of air by it be rendered evident: as the coating from a square yard would only cover a square inch to the depth of the 34th part of an inch, and as the real diameter of particles of water may be hundreds or thousands of times less than that adopted for this calculation, the coatings of electricity necessary to render them buoyant may be almost infinitely less than the quantity here given. That this is not an exaggerated idea of what may be the extreme minuteness of a particle of water, is shown by the following extract from Ehrenberg, who states, that "he has grounds for believing the walls of monadal stomachs to be from about the 4,800,000dth to the 6,400,000dth of a line in diameter," that is, from the 57,600,000dth to the 76,800,000dth of an inch in thickness; and if living animal matter can be of such extreme tenuity, what may

ticles of water of which it is probably in part composed[g]?

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[g]" *On the Magnitude of the Ultimate Particles of Bodies.*
By Prof. C. C. Ehrenberg.

" *Extraordinary Minuteness of the Infusoria.*—My observations, in regard to the smallest organic parts, have enabled me to ascertain the following smallest magnitude, as actually existing and discernible by the senses:

" By means of the microscope, I saw distinctly monades in which the greatest diameter of the body was from $\frac{1}{1500}'''$ to $\frac{1}{2000}'''$ of a line. This, which is the smallest of known animals, I have named *Monas termo*, is the same as that described by Otto F. Muller, under this name. In the largest individuals of this animal, I was able, by colouring the liquid, to discover in some, the larger, six, in the smaller, four sacs or stomachs, and in some of them the stomachs did not occupy the half of the whole animal. Such a stomach of the *Monas termo* therefore, if the animal is only $\frac{1}{1500}$ part of a line in size (if there are only four stomachs which occupy the half of the animal), is $\frac{1}{12000}$ part of a line in magnitude. We observe in the forepart of these animals, as in all the monades, a violent projection of still smaller bodies than itself, as soon as these come near to it, hence these have probably a wreath of ten or twenty feelers around the anterior mouth opening, as in the *Monas pulvisculus*, and the other still larger monades. It is probable that each of the stomachs which are filled in our experiments with colouring matter, contain more than one atom; if each stomach contains three coloured atoms, this affords a proof of the existence of red and dark blue particles of colouring matter floating in water, with a magnitude of $\frac{1}{36000}$ part of a line, $\frac{1}{432000}$ part of an inch in diameter; and if the same objects are calculated according to the smallest animals we have observed, which are $\frac{1}{2000}$ of a line in magnitude, and sometimes contain four coloured points in the hinder part of the body, these latter parts, which are no longer individually distinguishable, even by a power of 800, but are distinguished in the aggregate, have a magnitude of $\frac{1}{48000}$ of a line, or $\frac{1}{576000}$ of an inch. We may also notice the fineness of other parts of these *living organic beings*. The small stomachs of the monas appear isolated in the body, and sharply bounded. In larger infusoria

73. With respect to the weight of electricity, I have already stated that I do not consider it necessary for the truth of the theory that electricity should be ponderable,

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which are $\frac{1}{48}'''$, or upwards, in diameter, we see these receptacles as distinct bladders, and there is no reason for assuming another structure in the coloured cavities observed in the smallest of the monades in our experiments. If we assume the thickness of the walls of the stomach as $\frac{1}{10}$ of its diameter, it amounts in the *Monas termo*, having a diameter of $\frac{1}{2000}'''$, where the stomach appears as the $\frac{1}{8}$th part of the measurable length of the whole animal, consequently $\frac{1}{16000}'''$ in diameter, to $\frac{1}{160000}$ of a line or $\frac{1}{1920000}$ of an inch, and as there is reason for supposing that the walls of the stomach contain vessels, it affords a still further minuteness of atoms. But magnitudes even smaller than these may be pointed out. In the *Polygastric infusoria* there is an ovarium. The grains of this ovarium are as 40 to 1 in the female of the *Kolpoda cucullus*, in others as 80 to 1; and they appear to increase in fineness as the body diminishes in magnitude. Is it not probable that it is only the transparency and the imperfection of our microscopes that prevents us from observing a similar ovarium in the monades, which are similarly organized, so that it cannot be overlooked that there may be young monades contained in the ovum, or which have escaped from it, in which the diameter of the whole body measures only $\frac{1}{60000}'''$ to $\frac{1}{80000}'''$, and which also are provided with stomachs, which, according to the same relation, will have a diameter of from $\frac{1}{480000}'''$ to $\frac{1}{6400000}'''$? The walls of these monadal stomachs will be about $\frac{1}{4800000}'''$ to $\frac{1}{64000000}'''$ in diameter.

" By the kindness of Professor Ensler of Berlin, I have been enabled to make many observations with a solar microscope. On viewing the *Monas atomus*, strongly filled with indigo, I discovered in the intervals the shades of smaller monades, which could not have amounted nearly to $\frac{1}{2000}'''$, but which were quite invisible in the water when examined with Chevalier's microscope, perhaps on account of their transparency. Whether these bodies were the young of the *Monas atomus*, or independent species, it follows that $\frac{1}{2000}'''$ is not the limit to a size of organic forms which can be distinctly seen."—*Edinburgh New Phil. Journal*, October, 1832.

and I believe it is not subject to the ordinary laws of gravitation. But be this as it may, the foregoing calculations will show the improbability of its weight being ascertainable, and there would also be a difficulty from the attractive or inductive effects of surrounding objects on the charged body to be weighed.

74. The passage of electricity through dense bodies tells but little against its materiality, as the pores of metals, although too small for the admission of air or other gases, may freely admit the electric fluid; and the freedom with which it pervades metals may be owing to the absence of air, which is one of the greatest opponents of electric motion.

75. There is something analogous to this in the fact that a vessel already filled with gas will receive an addition of gas of another kind, without compressing that already in it; the one gas being as it were a vacuum for gas of another sort.

76. It is probable that metals are the best conductors, not from their attracting or having an affinity for electricity, but from their offering the least resistance to its passage; therefore a good conductor may be a sort of vacuum as far as regards electricity. With this view of the question, it is not surprising that electricity should take a course through an enormous length of good conductors, rather than force a passage, even for a very short distance, through a bad one; as water will run through a long pipe, if of sufficient dimensions, although formed of slight materials.

77. An experiment of Dr. Faraday's has been mentioned to me several times as a proof of the non-materiality of electricity, which is, that small Leyden jars of glass have been electrically charged, and then hermetically sealed; but when examined a year or two afterwards, they have been found to have no charge whatever remaining in them. I do not think this proves the non-materiality of electricity, for although glass may resist the passage of electricity to a very high degree, it is still possible that it may be slowly permeable by it; as vessels may be made of mate-

rials which will hold water well, but not altogether prevent it passing away, perhaps imperceptibly, in the course of time.

78. Many are led to doubt the materiality of electricity, from the inconceivable velocity with which it passes through air from one body to another, or through solid conductors: still all rates of speed are but comparative; and when witnessing the destructive effects of lightning, it seems to me more difficult to look upon them as the effects of a mere change of condition in, or undulation of the molecules of the bodies affected, rather than as caused by the passing of a material agent, even with such extraordinary velocities. And the seemingly instantaneous effect produced through solid conductors of great length, such as the Atlantic telegraphic cable, may be owing in some degree to the forcing onward of electricity already, and at all times, within the conductor; as water, if forced into a pipe already filled, will at the same time drive out an equal quantity at the other end of the pipe, be it ever so long. This seems to me a more probable way of accounting for the phenomenon than to suppose the whole of the molecules of the wire to be vibrated or polarized from the effects of a battery at the one end of it.

79. Looking at the subject in all its bearings, the intense velocity of electricity in motion cannot be admitted as a *proof* that it is not material, and the following remarks by M. Arago show the doubt and uncertainty in which the question is involved.

80. "From the earliest times it has been known that sound was not material. Thus, Aristotle knew perfectly well that it was produced by simple undulations of the common air. At the present time, this result may, without scruple, and with a single modification, be extended to light; for light also is the consequence of the undulatory motion, not of air, but of a certain very rare and very elastic medium which pervades the universe, and which it has been agreed shall receive the name of *ether*."

81. "Are we to range the thunderbolt, whose presence is almost always manifested simultaneously with light and

sound, in the same category? Though free to declare myself the decided partizan of the theory of the undulation of light, I must avow I remain completely undecided on this other point. When on the one hand I consider the observations of Mr. Wheatstone as completely established, and when I direct my attention to the incomparable velocity with which the thunderbolt traverses the aërial regions, and the solid bodies which propagate it at the surface of the earth, I feel myself but little inclined to regard it as composed of an agglomeration of material molecules, or a mass of minute projectiles, and the idea of undulations seems most readily to comport with such extraordinary velocities. But no sooner, on the other hand, have I reached those conclusions, than the prodigious mechanical effects produced by thunderstorms, and the transport of the heavy substances thereby effected, rush to my recollection. Then all my uncertainty returns, and the *undulations of the thunderbolt appear enveloped in ten thousand difficulties.*"—*Edinburgh New Phil. Journal*, vol. li. p. 81.

82. On the question of electricity coating the surface of bodies few remarks are necessary, as I believe it is generally considered as proved, that the capacity of a body for electricity is in proportion to its surface, rather than its solid contents; although it may pervade the pores of all bodies also.

83. I have no idea that what I have advanced will *prove* that electricity is material; my object has been to show that there is *no proof* that it is not so; and that it may occupy space, although we have no means to prove it: therefore that the theory I propose ought not to be rejected on that ground alone. I fear I have not rendered my meaning so clear as I could wish to do, and I would gladly have avoided this part of the subject altogether, but for the fact that the question has often been asked me, How can electricity have a buoyant power, and be the cause of the ascent of vapour, when no difference can be detected in the weight of an insulated body when charged or not?

On the Connection of Electricity with Meteorological Phenomena.

84. Before I attempt to show the adaptation of the theory to any particular phenomenon, it may be well to direct attention to the general connection of electricity with meteorological phenomena, and that (apart from any theory) it is developed in all cases.

85. From the time of Franklin the electricity of the atmosphere has attracted special attention, and I shall have to allude to the observations of many persons in reference to the subject. But of all the experiments and observations on atmospheric electricity which have, at various periods and in different countries, been carried on by so many eminent men, none seem to have been conducted with greater care, patience, and perseverance, than those of the celebrated Giambatista Beccaria, Professor of Natural Philosophy in the University of Turin. His observations were not confined to one locality; he was careful in selecting lofty and open situations, where his experiments would as far as possible be uninfluenced by neighbouring objects, and he seems to have had no theory in view which could bias his conclusions. The experiments are very numerous, and many of them have special bearing on the subject of this Essay. The results could most readily be exhibited in an epitome, but as that might be considered partial, I prefer giving extracts from his book. These I have placed together, and for convenience of reference the numbering of the paragraphs is given from his work, in addition to the running number of this Essay.

86. On the question of the electric condition of the atmosphere during clear weather he dwells at considerable length (408), and says (412), "Ever since I began to observe the atmospheric electricity during *serene* weather, the whole series of my observations has confirmed it to me, that this electricity is constantly of the excessive, or positive kind;" and he further states, that during the

many years he carried on his observations, he only met with six instances of defective (negative) electricity; that on each occasion he saw clouds at a distance of a peculiar form, and shows that these six cases were evidently the result of induction; as he says (417) that these instances were similar to those in which, "though the sky just over head may be clear, dark thick clouds, which draw near to the place of observation, send an electricity which is found to be alternately excessive and deficient."

87. With respect to the presence of electricity in the atmosphere, Beccaria shows (422) that it is only in connection with the vapour floating in it, and says (429), "Whence we are to conclude, that the quantity of electric fire which is exerted in the atmosphere during clear weather is proportioned to the quantity of moisture contained in it." He shows also (430) that fogs give off electricity, and (433) states, "that he finds by experience that all the vapour, or moist effluvia whatever, which are anyhow brought to rise in the atmosphere, or which swim, or descend in it, are affected by the aërial electricity, in their absolute as well as relative motions;" and concludes this part of the subject thus: "The natural electricity of the atmosphere during clear weather resides chiefly in the vapour diffused in it."

88. As to the opinion that the friction of the air is the cause of the development of atmospheric electricity, he condemns it in toto (435), and devotes sixteen paragraphs to support the proposition that, "the friction of the winds against the surface of the earth is not the cause of atmospheric electricity; and that tempestuous winds used to lessen the electricity in clear weather." The remainder of the extracts (441) relate to the electricity developed during the deposit of dew.

89. The intimate connection of electricity with the more striking of meteorological phenomena is obvious, for not only do thunderclouds produce electric effects, but all other clouds do so in proportion to their density, or proximity to the earth, as shown by their effects on inductive rods, electric kites, &c. Hail-storms are generally accom-

panied by lightning, and rain and snow are often found to be electric.

90. On the question of the electricity of clouds, many are of opinion that they differ in their electric state, and that some are positively and others negatively charged; and even that the condition of the same cloud may vary from the one to the other of these electric states. Mr. Cross describes the effects of a passing thundercloud as exhibiting alternate signs of negative and positive electricity, and supposes the clouds to be disposed in concentric and oppositely electrified zones, the electric intensity decreasing from the centre towards the edge. I have only an opinion to offer against that gentleman's very extensive practical observations; but I cannot conceive the possibility of such opposite conditions existing at the same time in the same cloud, or how the electricity could accumulate in the cloud so as to strike off to the earth, while the opposite electric conditions exist within it.

91. On the theory I propose a cloud can only be suspended by its positive charge of electricity, and I believe that all the negative effects of clouds may be accounted for as inductive.

92. The effects of a thundercloud seem to be in accordance with the generally acknowledged laws of electric action; as on its approach as a body highly charged positively, it may give negative signs by induction, while from the same cloud when over head, especially if it be raining heavily, the electricity passing off to the earth may produce positive effects; the signs again becoming negative from induction as the cloud passes away. The signs may even vary while the cloud is over head, and must do so to some extent; for a certain degree of intensity is necessary for the electricity to overcome the resistance of the air, therefore the electricity accumulates in the clouds till a flash of lightning takes place, which must at the moment affect the electric tension of the cloud, and cause its effects to vary[h].

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[h] Such varying effects seem to have been felt by the writer of

93. Other clouds may at times give negative indications, but all fixed atmospheric electric apparatus, be they

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the following account, who appears to have been almost in the very cloud from whence the thunder and lightning was produced :—

"*Observations on a Thunderstorm*, by L. Blesson, Major of Engineers, Berlin.

"On ascending a mountain, which rises rather more than 2000 feet above Teschen, I encountered a storm, concerning which the following particulars are not without interest. The wind blew from the south, and, shortly after I commenced my ascent, enveloped the upper part of the mountain in clouds. The oppressive feel of the air seemed to announce a coming thunderstorm, but hitherto neither thunder nor lightning had occurred. The nearer I approached to the clouds, the darker was their colour, but still the sun shone brightly upon Teschen. The clouds, as seen from below, which exhibited a remarkable rotatory motion, appeared sharply bounded, and I was therefore surprised, when I came near to them, to find, as usual, only a gradually denser and denser cloud, which speedily wet me through. A particular rotatory wind appeared to prevail in this region (above half-way up the mountain), occasioning a piercing cold, which was the more striking, as contrasted with the sultry heat and stillness below the clouds.

"I had hardly entered the denser part of the cloud, where it was so dark that I could with difficulty distinguish an object at my foot—(I name this *dark*, because I do not know any other expression for it; it is not, however, want of light; we have a white veil before us, which is constantly moving with a rotatory motion, which we cannot compare with anything else). I was scarcely in the cloud before I felt throughout my whole body a kind of expansive tension, which was excessively oppressive, and seemed to affect the walking of my companion, a poodle dog, even more than it did myself. The hair appeared to bristle up, and it seemed to me as if something was drawn out of the whole of my body. But this electric tension was of a very different character from that of an isolator. I bent down, in order to see the grass that surrounded me, and on which no dew was ob-

wires or induction rods, are so slightly elevated in comparison with the generality of clouds, that it seems impossible to decide by them alone whether the effects are inductive, or indicative of the real condition of a cloud. All the observations of Beccaria, to which I have alluded, show that the normal electric condition of atmospheric vapour is positive; I believe that no one has ever found

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served, when I was suddenly enveloped in a bright sea of light, with a yellow lustre, and perceived, along with a violent noise, a sudden cessation of the former tension. The noise may be best compared with a distant dull cannon-shot, only more continuous and louder, or may be compared with the explosion in a mine; but no rolling was heard. The grass was in motion, but I was too much surprised and confounded to make more particular observations. The convulsive motion of the cloud ceased for a moment, but immediately began again, and with it the tension was renewed. During the moments of rotation, the vaporic particles appeared to be arranged in rows into fibres, which moved still more violently amongst each other, and after the explosion all was again calm, and a mere fog or cloud was visible. My poodle dog was the first object of my attention; it seemed to me to be thicker than usual, and his hair bristled up; I stroked it several times, and saw it bristle up under my hand. A new flash of lightning took place, and I could distinctly perceive, notwithstanding the light, that the whole body of my dog glimmered with a peculiar lustre, the hair, formerly bristled up, now fell flat, and he sunk down on his knees. This was a consequence of the stronger streaming of electricity from him than I experienced, and which seemed, as it were, to draw me from the mountain. Although during the tension, the feeling of drawing out was continuous and always increasing in intensity, still it was strongest at the moment when the electrical discharge took place; the hair bristled up more, and I felt something, as it were, passing from out my interior, and instantaneously all was past, and the hair flat again. On the next flash of lightning, I noticed the appearance of the grass; on the discharge it appeared shining at its extremities; it became erect when I felt the tension increasing in my body, but became gradually wet, and then sunk down again."—*Edinburgh New Phil. Journal*, January, 1833.

negative signs from a cloud when at a moderate height over an electric kite, whatever may have been the indications from the cloud when approaching the kite; and the following extract tends to show the probability that the negative effects of clouds are at all times from induction, and not from the clouds actually being negatively electrified.

94. "During a period of three years and seven months, viz. from January 1844 to July 1848, both inclusive, electrical readings were taken at the Kew Observatory, at each even hour of Greenwich mean time, as well as at sunrise and sunset. This long series of observations amounts to 10,526 readings. Of these 10,176 were positive, 324 were negative, giving 31 positive to 1 negative indication. The great majority of instances in which negative electricity was exhibited were characterized by two very interesting features. At Kew one of these features was the falling of rain, in most instances heavy, and the other the occurrence of cirro-strati, and occasionally of cumulo-strati, which clouds were considered to have contributed their quota to the development of the electricity observed. Although the small number of observations did not furnish data sufficient to determine a diurnal period, they *pointed out a connection between negative readings and a prevalence of clouds,* when there was no rain; for though negative electricity is *generally,* it is not always, accompanied by the falling of rain, nor is all rain accompanied by negative electricity."

95. "The observations of M. Quetelet at the Royal Observatory at Brussels, from August 1844 to December 1848, gives the like results, as 'during the whole period included in the register, the electricity was observed to be *negative* only twenty-three times, and these indications *generally* either *preceded* or *followed rain* or *storms.*'"—*Noad's Manual of Electricity,* pp. 188, 190, 194.

96. No really satisfactory knowledge of the electric condition of clouds can be obtained unless by the aid of captive balloons, by which conducting wires could be carried up to the clouds themselves; although experiments with

kites (being of an exciting character) have, from the time of Franklin, been carried on with considerable success in various countries, and by many persons of eminence in scientific pursuits. As these experiments are pretty well known, it may be unnecessary to enter into detail on the subject; still I think it may be well to refer to some of the results. Of all such experiments none seem to have attracted so much attention, or to have been of such an exciting character, as those of M. de Romas: and a fair consideration of them will show, in a strong light, the probability that electricity is an important agent, rather than a minor result in meteorological phenomena; that the accumulation of electricity to such a degree cannot be attributed to the mere friction of air on vapour; and I believe few persons could have witnessed such effects without being convinced of the materiality of the electric fluid, or, at all events, doubting its non-materiality.

97. "M. de Romas, Judge of the Presidial Court of Nerac, in Aquitain, was the first who used a wire interwoven in the cord of an electric kite, which he made seven feet and a half high, and three feet wide. This cord was found to conduct the electricity from the clouds more powerfully than an hempen cord could do; and being terminated by a cord of dry silk, it enabled the observer (by a proper management of his apparatus) to make experiments without danger to himself.

98. "By the help of this kite, on the 7th of June, 1753, about one in the afternoon, when it was raised some 550 feet from the ground, and had taken out 780 feet of string, making an angle of near forty-five degrees with the horizon, he drew sparks from his conductor three inches long and a quarter of an inch thick, the snapping of which was heard about 200 paces. Whilst he was taking these sparks he felt, as it were, a cobweb on his face, though he was above three feet from the string of his kite; after which he did not think it safe to stand so near, and called aloud to all the company to retire, as he did himself about two feet.

99. "Thinking himself now secure enough, and not

being incommoded by anybody very near him, he took notice of what passed amongst the clouds which were immediately over the kite, but could perceive no lightning either there or any where else, nor scarce the least noise of thunder, and there was no rain at all. The wind was west, and pretty strong, which raised the kite 100 feet higher, at least, than in the other experiments.

100. "Afterwards, casting his eyes on the tin tube, which was fastened to the string of the kite, and about three feet from the ground, he saw three straws, one of which was about one foot long, a second four or five inches, and a third three or four inches, all standing erect, and performing a circular dance, like puppets, under the tin tube, without touching one another.

101. "This little spectacle, which much delighted several of the company, lasted about a quarter of an hour; after which, some drops of rain falling, he again perceived the sensation of the cobweb on his face, and at the same time heard a continual rustling noise, like that of a small forge bellows. This was a further warning of the increase of electricity; and from the first instant that M. de Romas perceived the dancing straws, he thought it not advisable to take any more sparks, even with all his precautions, and he again entreated the company to spread themselves to a still greater distance.

102. "Immediately after this came on the last act of the entertainment, which M. de Romas acknowledged made him tremble. The longest straw was attracted by the tin tube, upon which followed three explosions, the noise of which greatly resembled that of thunder. Some of the company compared it to the explosion of rockets, and others to the violent crashing of large earthen jars against a pavement. It is certain that it was heard even in the heart of the city, notwithstanding the various noises there.

103. "The fire that was seen at the instant of the explosion had the shape of a spindle eight inches long and five lines in diameter. But the most astonishing and diverting circumstance was produced by the straw, which

had occasioned the explosion, following the string of the kite. Some of the company saw it at 45 or 50 fathoms distance attracted and repelled alternately, with this remarkable circumstance, that every time it was attracted by the string, flashes of fire were seen, and cracks were heard, though not so loud as at the time of the former explosion.

104. "It was remarkable that from the time of the explosion to the end of the experiments, no lightning at all was seen, nor scarce any thunder heard. An end was put to these experiments by the falling of the kite, the wind having shifted into the east, and rain mixed with hail coming on in great plenty."—*Gentleman's Magazine* for August, 1756, p. 378.

105. The following interesting account of a similar kite experiment by Mr. W. Sturgeon is taken from the *London and Edinburgh Phil. Magazine* for Dec. 1834:—"On Saturday the 29th of March I had a very favourable opportunity of demonstrating experimentally to my friends at Kirby Lonsdale, that an abundance of the electric fluid usually attends hail and snowstorms. The wind was pretty brisk, cold, and from the west by north, nearly the whole of the day. There were several hail showers, each of which, with a simultaneous increase of wind, became a complete transient storm. During three of these hailstorms I floated one of my silken electric kites, with a wired string of about 300 yards long, and insulated in the usual way by means of a silken cord.

106. "The kite was elevated in each experiment about ten minutes prior to the arrival of the hailstorm, and the electric state of the atmosphere ascertained, which was found to be so exceedingly feeble that not the slightest spark could be observed. As, however, the cloud from which the hail was falling approached the kite, the fluid from the string presented itself in brilliant sparks to the knuckle; and during the transit of the cloud, became so abundantly discharged to a wire presented to the string, that it struck in rapid succession through a stratum of six inches of air; and through three inches of air, it presented

a splendid continuous stream of electric fire. As the cloud receded from the kite, by advancing in its aërial course, the electric discharges became less and less brilliant, and continued to diminish in splendour and energy with the recession of the passing storm, ultimately vanishing altogether by the emergence of the kite from the electric influence of the cloud.

107. "These appearances were exhibited in each experiment, but the display of the electric fire was the most magnificent in the second, which was the fiercest hail-storm of the day, and happened between two and three in the afternoon. During an early part of this storm the electric fluid made a continuous rattling noise down the kite-string (in consequence of the wire being broken in several places), and darted from the reel at the inferior extremity, to greater distances than in either of the other experiments. In one instance it struck over a stick a yard long, to the hand of a young man named Croft, who was presenting it to the kite-string. Although the remote end of the stick was in connection with the ground by means of a very wet string, and consequently a considerable discharge must at the same time have passed down the wet string to the earth, the shock was so violent as to make Mr. Croft reel and nearly fall; and I have some reason to suppose that it has left an impression on his memory which time will not speedily obliterate. The kite-strings, however, broke soon afterwards, and consequently the experiments, on that occasion, terminated very abruptly; and unfortunately at a time also when the fire was streaming from the string in the greatest abundance, and with a degree of splendour better imagined than described."

108. The foregoing relates to the electricity of what may be called *hail showers:* to its connection with the more destructive storms I need scarcely allude, as my object here is only to show that electricity plays a part in all meteorological phenomena; and the following shows that this is the case in a phenomenon which I should have thought might have been attributed to heat alone.

109. The subjoined extracts are from a letter in the *Phil. Mag.*, vol. xxxvii, *On the dust storms of India, by P. Baddeley, esq.*, dated Lahore, April 18, 1850. "My observations on this subject have extended back as far as the hot weather of 1847, when I first came to Lahore, and the result is as follows:—Dust storms are caused by spiral columns of the electric fluid passing from the atmosphere to the earth; they have an onward motion—a revolving motion, like revolving storms at sea."———My idea is, that the phenomena connected with dust storms are identical with those present in waterspouts and white squalls at sea, and revolving storms and tornadoes of all kinds; and that they originate from the same cause, viz. moving columns of electricity.

110. "In 1847, at Lahore, being desirous of ascertaining the nature of dust storms, I projected into the air an insulated copper wire, on a bamboo at the top of my house, and brought the wire into my room, and connected it with a gold-leaf electrometer and a detached wire communicating with the earth. A day or two after, during the passage of a small dust storm, I had the pleasure of observing the electric fluid passing in vivid sparks from one wire to the other, and of course strongly affecting the electrometer. The thing was now explained; and since then I have, by the same means, observed at least sixty dust storms of various sizes, all presenting the same phenomena in kind. I have commonly observed that, towards the close of a storm of this kind, *a fall of rain suddenly takes place,* and instantly the stream of electricity ceases, or is much diminished.

111. "The sky is clear, and not a breath moving; presently a low bank of cloud is seen in the horizon, which you are surprised you did not observe before; a few seconds have passed, and the cloud has half filled the hemisphere: and now there is no time to lose—it is a dust storm, and helter skelter every one rushes to get into the house in order to escape being caught in it.

112. "The electric fluid continues to stream down the conducting wire unremittingly during the continuance of

the storm, the spark oftentimes upwards of an inch in length, and emitting a crackling sound; its intensity varying upon the force of the storm, and, as before said, more intense during the gusts."

113. "Many dust storms occur at Lahore, in the Punjaub, generally during the hot and dry months, as many as seven or nine in one month.

114. "The instant the wire is involved in the electric current marked by the column of dust, down streams the electricity. I have sometimes attempted to test the kind of electricity, and find that it is not invariably in the same state; sometimes appearing +, at other times —, and changing during the storm."

On the Cause of Evaporation, Rain, Hail, Storms, &c.

115. Assuming that electricity occupies space, has no weight, and coats the surface of bodies, I will now endeavour to show that the theory proposed (46) will afford a fair explanation of the causes productive of rain and *all* its allied phenomena.

116. Before entering into particulars respecting the various phenomena, I would direct attention to the following table of the comparative weight of water and air at different elevations above the level of the sea, and the temperature at such heights.

Height.	Temperature of Air.	Density of Air.	Water heavier than Air.
5 miles	— 25° Fah.	0·3163	2719 times
4 miles	— 8°	0·3981	2160 ......
3 miles	+ 9°	0·5011	1716 ......
2 miles	+ 26°	0·6309	1363 ......
1 miles	+ 43°	0·7943	1083 ......
Level of the sea	+ 60°	1·	860 ......

Barometer at the level of the sea 30 inches.

I believe it is by no means a settled point as to what

is the difference in the weight of water and air at various heights, as some give it at the level of the sea as not more than 820 times; and the decrease of temperature at different elevations is by no means certain. I have adopted the rates given in several meteorological works, and as the table is merely to elucidate the theory, it is not necessary that it should be absolutely correct, and I believe it is quite near enough for the purposes required.

On Evaporation.

117. The capacity of a body for electricity being in proportion to its extent of surface, rather than to its solid contents; as the surfaces of bodies increase in proportion to their bulk as the bulk itself diminishes, there must be a point at which the surface would be so great in proportion to the mass, that let the specific gravity of a body be ever so great, it must be rendered buoyant when enveloped in a coating of imponderable matter.

118. To put this case stronger, let us take a cubic foot, and it has six times 144 inches of surface, but there are twelve times 144 cubic inches contained in it; therefore there is only half an inch of surface to every inch of solid contents; but if we divide the body into cubes of one inch diameter, then there will be six inches of surface to each inch of matter; and if a further division be made into cubes of the twelfth of an inch, the surface will be further increased to 72 to 1, and thus division on division may go on till a body may be nearly all surface, and its capacity for electricity increased almost to infinity. Again, a large mass of heavy metal in water would be but little affected by a very thin coating of cork, whereas small particles of the same metal would be freely buoyant with a similar coating.

119. Thus, in accordance with the theory, I believe evaporation at low temperatures is caused by the particles of water being so small, that when detached from contact with other particles, and consequently being completely

enveloped in their natural coating of electricity, they are thereby rendered sufficiently buoyant to be carried off by currents of air. But when, in addition to these circumstances, the particles of water are expanded by heat, they take up electricity in proportion to their expanded surface, and are buoyed up into the air by their increased charge of electricity.

120. Before entering into particulars respecting the various phenomena of evaporation, I will refer to the proofs that electricity is an agent in producing it. From the time that Franklin proved the identity of lightning with electricity, it was believed by many that evaporation was the source of atmospheric electricity. Volta succeeded in showing that electricity passes off during evaporation, and the experiment is still known in connection with his name. It is as follows:—If a hot cinder or piece of metal be placed on the cap of a gold-leaf electrometer, and a few drops of water thrown upon it, the leaves will diverge with negative signs at the moment the vapour is evolved, showing that positive electricity passes away with the vapour.

121. Mr. H. L. Pattinson gives the following in the *Phil. Mag.*, Dec. 1840, p. 460, in reference to his investigations on the cause of the electricity of steam:—"I repeated Volta's experiment by placing a hot cinder upon the cap of a gold-leaf electrometer, and projecting a few drops of water upon it, when the leaves diverged strongly with negative electricity. I observed that when the cinder was very hot, and the production of the steam consequently very rapid, the electricity given out was always most powerful.

122. "I then insulated an iron pan, twelve inches diameter and two inches deep, and attached to it a pith ball electrometer, with balls three-eighths of an inch diameter, and threads five inches long; and also attached to the pan a metallic wire, the pointed extremity of which was placed about one-twentieth of an inch distant from the point of another wire connected with the ground. The iron pan was then filled with cinders, very hot, from a wind-furnace,

and on projecting upon them a few ounces of water, steam was evolved with great rapidity, and at the same moment the pith balls diverged to the distance of an inch, and sparks passed between the metallic wires. This was repeated several times."

123. The proof of the electricity of steam, as discovered in 1840, seemed to be a full and perfect development of Volta's experiment. It created but little surprise, as it had long been suspected that steam was electrical. I had several times tried (and at some personal risk) to produce electricity by condensing the steam from a common kettle, but of course without success, owing to the conducting properties of ordinary steam.

124. From the first discovery I believed, and still hold, that the electricity of steam affords strong evidence in support of the theory I propose; and especially the fact that the *same* jet of steam exhibits three electric conditions. Thus (as shown by Professor Faraday), if an insulated body be held in the jet, near the orifice from which the steam is escaping, it is brought into a negative condition; *at some point* at a little distance from the boiler the jet is in a neutral state, but at a still greater distance the steam is positively electrified. This phenomenon seems to admit of a simple explanation. At the moment the steam escapes from the boiler it expands enormously; it is therefore increasing its capacity for electricity, and anything held in it is robbed of its electricity, and brought into a negative condition: between this point and the other extreme there must be a neutral part; but still further off, where the steam is condensing, consequently losing its capacity for electricity, from the contraction of the particles of steam, it is there brought into a surcharged or positive state.

125. The electricity of steam has been attributed to friction of the particles of water carried along by the steam, and rubbing against the sides of the exit tube. This opinion I controverted in an article in the *Edinburgh New Phil. Journal* (1844), and I know that my views

were approved of by many persons of eminence in the scientific world.

126. It was known long since that the evaporation of water and the like is increased by its being electrified. This was proved a hundred years ago by the Abbe Nollet, who showed that "electricity augments the natural evaporation of fluids;" and that "electricity has a greater effect upon fluids when the vessels which contain them are non-electrics, the effects always seeming to be a little greater when the vessels were of metal than when they were of glass."

127. Cavallo also, in his Treatise on Electricity, states that electricity, "if communicated to insulated fruits, fluids, and in general, to every kind of bodies that are in a state of evaporation, it increaseth that evaporation, and in a greater or less degree as those bodies are more or less subject to evaporate themselves, and as they have a greater or less surface exposed to the open air."—*Annals of Electricity,* vol. viii. pp. 180. 182.

128. In accordance with the proposed theory I was led to think that evaporation would not go on so freely from an insulated vessel as from an uninsulated one, and in 1841 I tried several experiments, the following account of which appeared in the *Phil. Mag.,* Jan. 1842. "In a warm room, over an oven in daily use, I suspended with silk threads two shallow vessels, eight inches and a half in diameter, containing eight ounces of water each; a small copper wire was hung from one vessel to the earth to take off the insulation, both vessels being similarly suspended in every other respect: after being suspended twenty-five hours the insulated vessel had lost two ounces, eleven dwts., and fifteen grains; and the other vessel three ounces six dwts., showing an excess of evaporation from the non-insulated vessel of fourteen dwts. nine grains. I have tried similar experiments with water placed in the rays of the sun, and on all occasions the evaporation has been greatest from the non-insulated vessel."

129. I have described the room in which the above

experiment was made as warm, I might have said hot, but I did not take the temperature. I have since then tried similar experiments at more moderate temperatures with like results, but in a less striking degree. And in the *Annals of Electricity,* vol. viii. p. 325, Mr. T. Spencer, in an article on Atmospheric Electricity, after referring to the foregoing experiment, says, "I have repeated a similar set of experiments, and with nearly similar results; always, at least, showing an excess in favour of the non-insulated vessels of water."

130. Thus the agency of electricity in evaporation is shown in various ways; as electricity goes off during evaporation, an excess of it accelerates evaporation, and, as in the last experiments, the want of it retards evaporation.

131. Evaporation from water in its most condensed state I have already explained as caused by the particles of water, together with their electric coatings, being so near the weight of air as to be carried off and buoyed up by the winds, as other light substances may be; and that light bodies can be so suspended for a time, although in reality having a greater specific gravity than air, may be seen by the particles floating in a room if a ray of sunlight be admitted: such bodies will float for a long time if the air be but slightly agitated, but settle in the form of dust if the room be left undisturbed for a while.

132. Evaporation must depend on the surface exposed, and not on the volume of the evaporating fluid, as only the particles on the surface can obtain their coatings of electricity.

133. Wind increases evaporation by assisting the evaporating particles to separate from the body of water, thus enabling the particles to obtain their full coating of electricity, which they cannot have when in contact with any other body.

134. Evaporation from ice (22) is owing to the coldness and dryness of the air separating the minute particles at the surface, when obtaining their coatings of electricity,

they are rendered sufficiently buoyant to be carried off by a brisk wind. Evaporation from ice, snow, or even water at low temperatures, is trifling, except during windy weather.

135. The increase of evaporation with the diminution of atmospheric pressure, may be caused by the particles more readily separating from the surface of the water.

136. Evaporation in vacuo (i. e. under an exhausted receiver) is from the weight of the atmosphere being taken off, when the particles of water are buoyed up one upon another by their electrical coatings. This may appear an insufficient explanation of the cause, but I believe no one who has witnessed the surprising effects of electricity in vacuo, as exhibited by the aid of Ruhmkorff's induction coil, can doubt the sufficiency of electricity to produce such effects, however they may dispute the grounds on which I explain the phenomenon.

137. I am not aware of any experiments on the subject having been tried, and if not, it may perhaps be possible to show electric effects from evaporation in vacuo. By an arrangement somewhat similar to that in Mr. Pattinson's experiment (122), it is not improbable that the spark between the points of the wires might be produced, or it may be possible that a luminosity might be exhibited from the rising of the particles within the receiver, especially if evaporation could be prevented by mechanical means till the receiver is nearly exhausted of air.

138. As heat expands the particles of water, it increases their capacity for electricity; therefore, all other circumstances being alike, the greater the heat the greater its evaporation.

139. Here it may be well to consider the different effects of heat, as attributed to it in this theory, and those to which I have alluded. In the theory that evaporation is caused by the absorption of latent heat, it is assumed that water in evaporating takes a form materially differing from its previous condition, or that the heat absorbed expands the particles so enormously as to render them buoyant in air.

140. On the theory now proposed no such enormous expansion is necessary. A coating of less thickness than four and a quarter diameters of the particle would render it sufficiently buoyant to be carried off by the wind, as in evaporation from ice, or water at very low temperatures, as such a coating would give a buoyant power of 857, water being 860 times heavier than the air at a temperature of 60° at the sea level: this may be considered the natural coating of the particle, and an expansion of one quarter of a diameter of the particle would give a buoyant power of 1000, sufficient to make it float at about a mile high, which is considerably above the ordinary height of clouds (156); therefore a less expansion would suffice for raising vapour to the height of lower clouds, and the rising of vapour to greater heights may be further accounted for (160).

On the Cause of Dew, Rime, &c.

141. Vapour when rising thus expanded, if condensed by any object, would become surcharged by the contraction of its surface, when being attracted by the condensing body it would form dew or rime.

142. There can be no doubt that the deposit of dew is caused, as shown by Dr. Wells, by the condensation of vapour from the lower stratum of air, owing to the coldness resulting from the radiation of heat from terrestrial objects; still we have the experiments of Beccaria, which show that electricity is developed during its deposit.

143. Trees often drip with water at a time when there is no rain, or apparently any other precipitation of vapour; and this to a degree which seems more in accordance with the effects of attraction than of mere condensation: while the forms in which rime is deposited on the points and edges of leaves, &c. seem to be inexplicable except from the agency of electricity. The points and most prominent parts of objects are often fringed with rime, while the flat parts are quite free from it; this is particularly the case with iron bars and the like, and several interest-

ing examples of such deposits of rime are shown in the plates on meteorology in the *Encyclopædia Metropolitana.*

144. During the late autumn I saw some beautiful and interesting illustrations of this subject, in the form in which rime was deposited on the leaves and trees near the new University Museum. The ivy leaves being very smooth, were completely margined with a fringe of rime on all their edges, while the middle of the leaves was quite free from it; at the same time the dead leaves of the hornbeam, although intermingled with the ivy, being crumpled and rough, were completely covered with the rime, even on surfaces which were so inclined that the frozen particles could not have fallen on them.

145. The beautiful and delicate spicular form of snow crystals seems to favour the idea of electric action in the arrangement of the minute particles of which they are composed.

On the Cause of Fogs, and the Visibility of Vapour.

146. Fogs are known at times to be electrical. In the high and open situations chosen by Beccaria for his observations, they were positively electrified; but Mr. Cross and others have found them in both the negative and positive condition. This apparent contradiction does not in fact tell against the theory, as fogs are probably owing to different causes.

147. A fog may be caused by vapour rising freely, as from rivers, &c. being condensed at a slight elevation by the coldness of the air; when, from its proximity to the earth, and the conducting state of the atmosphere, it would at once lose its surcharge of electricity, and then sinking again amongst the still rising particles, would, together with them, become visible, and form fog.

148. Or in calm weather a fog may be formed by the gradual sinking of vapour floating in the air, as from the evaporation from ice, or water at low temperatures. Such fogs as these would probably give no sign of electricity, either negative or positive, as the particles would be in a like electric condition to the earth itself.

149. When a fog is rising, or rather evaporation is going on from a fog, from the direct effects of the sun or warm air, the negative sign would be exhibited, as the particles of vapour would then be expanding, and consequently taking up electricity. And fogs of any description may be rendered negative by the inductive effect of clouds.

150. But when fogs are caused by the condensation of the vapour of the lower air, from the cooling down of the earth's surface, then I believe that positive electricity only would be shown, if any signs were exhibited.

On the Visibility and Invisibility of Vapour.

151. I have already (21) stated my belief that there is really no essential difference in the condition of vapour when visible or invisible, and that its being invisible is simply owing to its being so diffused. I allude more particularly to vapour of the atmosphere, and not to steam generated at high temperatures. There is no proof of any cause operating during evaporation which can effect a change in the molecular condition of water: what is to produce such a change? Evaporation will go on from ice, or water at very low temperatures, and we have no reason to suppose that the process of atmospheric evaporation essentially differs in any way. A trifling increase of heat accelerates evaporation, a slight diminution of temperature reducing the vapour again into its previous condition, without any sign of chemical or other effects, except electrical, during the process.

152. On the other hand, dense bodies of vapour are visible at high temperatures, and yet the same vapour becomes invisible when diffused in air; the vapour of the breath of animals is visible close to the mouth, but invisible as it floats away to where it is more condensed but more diffused.

153. Glass, ice, &c. are transparent as water, but when minutely divided, the particles, in aggregates, are white and opaque, although each separate particle must be as transparent as the mass from which it was first broken;

and I believe that a particle of glass, if of the same form, and not bigger than a particle of vapour, would be equally transparent and invisible if floating in the air.

154. For the foregoing reasons I believe that vapours are at all times in reality the same; that is, particles of water coated more or less with electricity, and invisible at all times, except when the particles become within a certain limit of proximity. What these limits may be, I cannot guess; but it is a question which may perhaps admit of a mathematical solution, i. e. the comparative distances transparent globules must be from each other, to prevent such a refraction as would intercept the rays of light, and render particles of vapour visible.

On the Formation of Clouds.

155. Vapour rising into the air, and becoming condensed, would, if insulated, still retain its electricity, which would not only buoy it up, but, by forming an atmosphere around each particle, prevent the formation of rain; and the greater the expansion of the vapour on rising, the greater must be its coating of electricity when condensed, and the height at which it would be buoyant.

156. As the temperature is reduced at the height at which clouds are formed, vapour must be condensed at such heights, and the formation of clouds is generally not owing to the sudden condensation of the vapour, but to the escape of its electricity, thus allowing the particles of vapour to be brought nearer together by the attraction of aggregation, and thus become visible. Another cause may produce clouds; to which I will allude hereafter (159).

157. Clouds vary greatly in height, but "the inhabitants of level plains are less able to judge of the altitudes than those of mountainous regions. In Switzerland, for example, the mountains are often intersected in calm weather by horizontal bands of clouds, whose inferior borders are so uniformly terminated as to render the smallest change of altitude immediately perceptible. So constant, indeed, are these conditions of elevation, at times, that they remain for days suspended at nearly the

same height. Mountains therefore, or points on their surfaces, form a sort of scale by which at least the elevation of the denser sort of clouds may be measured; but we know of no example of its application, except the limits observed by Mr. Crosthwaite on Skiddaw; the comparatively low elevation of that mountain allowing a great proportion of clouds to pass above it. These observations, continued for five years, are contained in the following table."

Altitude of Clouds.	Number of Clouds.	Altitude of Clouds.	Number of Clouds.
From 0 to 100 yards	10	From 600 to 700 yards	416
100 200	42	700 800	367
200 300	62	800 900	410
300 400	179	900 1000	518
400 500	374	1000 1050	419
500 600	486	Above 1050 yards	2098

"Hence it appears, that the number of clouds above 1050 yards were to the number below that elevation, as 2098 to 3283, or as 10 to 16 nearly."—*Encyclopædia Metropolitana,* article Meteorology.

158. On the foregoing extracts and table I would observe, that the elevation of only the lower surface of clouds is given, and we have no knowledge of the depth of them. And as many clouds may have passed when hidden by clouds at a lower level, it is certain that the table does not give a fair estimate of the proportionate height of clouds; still it shows the low elevation of many clouds; and that they at times attain the height of five miles and upwards is shown by the observations of Humboldt, Boussingault, Gay Lussac, and others.

159. One ground for objecting to existing meteorological theories is, that they will not account for the ascent of vapour to great heights, and its suspension there when condensed. There can be no doubt that vapour is carried to great elevations by the *superior* trade winds, and from volcanos, &c.; still I believe that particles of water, or *vapour,* could not remain suspended for a moment at such

heights unless by the aid of electricity or some similar agent; and the vapour from such sources would not suffice for the rains in mountainous regions, or the heavy rain, hail, &c. which sometimes falls in lower districts, but evidently from great elevations.

160. I submit the following explanation on the subject. Vapour from the earth would rise with a coating of electricity in proportion to its expanded surface, *and in accordance with the electric state of the earth;* such vapour, if condensed and forming a cloud, would become surcharged with electricity in comparison with the earth. Now the sun shining on the cloud would cause evaporation from it, and the particles evaporating would rise from the cloud with a surface again expanded, *but coated with electricity in accordance with the surcharged state of the cloud,* and would consequently be buoyed up to a greater height than before; where another cloud may be formed, and thus the like process may be repeated again and again, and vapour be carried up to as great a height as it is ever known to attain. That evaporation does go on from clouds is certain, and I believe the process I have described is not an extraordinary one, but of continual occurrence, and is one of the most beautiful provisions of nature, being the means by which water is raised to the necessary height, from whence it is wafted away, till, intercepted by the higher mountains, it again falls, and passes off at once to supply the parched plains with moisture, or is locked up in nature's stores, the snows and glaciers, till the time of need.

On the Cause of Rain.

161. As various causes seem to operate in producing rain, I will take this subject under two heads, ordinary rain, and thunder rain: by the former I mean rain from thin and generally stratified clouds; by the latter, rain from cumulus clouds when of great depth, whether attended by thunder or not.

162. The precipitation of ordinary rain is, I believe, generally attributable to the escape of the surcharge of

electricity from the clouds, when the particles of vapour attracting each other form larger bodies, and fall as rain; therefore mountains or high hills cause rain by conducting the electricity from the vapour, and not by condensing it. Rain is also caused by the air between the earth and clouds becoming charged with vapour, so as to conduct the electricity from the clouds. Extensive fires, volcanos, &c. produce clouds and rain by the rising smoke, heated air, &c. conducting the electricity from the accumulated vapour and clouds to the earth; and I believe that anything that would conduct the surcharge of electricity from the clouds would cause rain: and therefore, in a paper read at the meeting of the British Association at Glasgow, in 1840, I suggested, as a mode of testing the theory, that the raising of electric conductors to the clouds by means of balloons, would enable the surcharge of electricity to escape, and thus cause rain to fall; and I have no doubt that, under favourable circumstances, clouds may be caused to form by withdrawing the electricity from the *invisible* vapour by similar means.

163. In confirmation of my views on this point I submit the following extract from a letter received from the late Mr. W. H. Weeks, of Sandwich (Dec. 27, 1842), a gentleman whose name must always stand high as an atmospheric electrician. The letter was sent to me in reference to my pamphlet *On the Cause of Rain, &c. with a suggestion for causing rain at will,* 1841.

164. "From very early life I have been an assiduous experimenter with electrical kites, atmospheric exploring wires, &c. Now I beg to assure you that it has several times happened, that when my kite has been raised immediately under a distended light fleecy cloud, at a moderate elevation, and a free current of sparks has passed from the apparatus during some ten or twelve minutes, I have suddenly found myself bedewed with a descent of fine misty rain, and on looking up have seen the cloud upon which I was operating surprisingly reduced in magnitude. The same thing has also occurred when experimenting with kites during the prevalence of a cirrus haze, on a

sultry summer's day. You perceive how intimately the above facts are connected with, or are rather confirmatory of your theory."

165. The above statement is strongly in favour of my views, not only as regards the theory, but also as to the importance of the proposed balloon experiment; for electric conductors raised by kites would not reach the clouds, and could only be raised in windy weather, when the clouds must every moment be passing away from such apparatus; and if conductors so raised could produce such results, much greater effects may be anticipated from the influence of conductors which could reach the level of the clouds, and be raised in calm weather.

On the Excessive Rains of Mountainous Districts.

166. Returning to a consideration of the effects of mountains in producing rain. The fact that they do so has been noticed for ages, but such effects were generally attributed to the lofty mountains of other countries; and quite a sensation was produced by an account of the enormous quantity of rain which falls amongst the mountains and lakes of Cumberland and Westmorland[k].

The following was the fall of rain in 1845:—

	inches		inches
Whitehaven	49·20	Westdale Head	109·55
Keswick	62·20	Grasmere	121·00
Loweswater Lake	69·54	Gatesgarth	124·13
Ennersdale Lake	76·88	Seathwaite in Borrowdale	151·87
Buttermere Lake	87·48		

Langdale Head 92·62 in seven months.

At Seathwaite there were 31 days on which the fall of rain was between 1 and 2 inches, 5 days between 3 and 4 inches, 1 day between 4 and 5 inches, and 1 day between 6 and 7 inches. Of the total rain measured in Borrowdale in 1845, 106·58 inches fell in the months of January, March, August, October, November, and December; nearly 46 inches of which was in the two latter months.

[k] Read by J. F. Miller, esq., at the Meeting of the British Association at Southampton, 1846.

167. The following returns of the rain in various places in 1845 will serve for comparison with the quantities in the lake district:—

	inches		inches
Kendal	53·34	Leeds	25·58
Manchester	41·41	Boston, Lincolnshire	24·29
Helston, Cornwall	37·80		
Liverpool	34·06	Kelso, Roxburghshire	24·42
Carlisle	31·28		
Davenport	29·81	Chiswick	23·30
Doncaster	29·19	Oxford[1]	21·41
Birmingham	28·95	Durham	19·80

The fall at Seathwaite was more than three times the quantity measured at Whitehaven, one of the wettest towns in the kingdom. It exceeded the fall at Leeds by six times, and that of Oxford by seven times.

168. "An inspection of a map of the country will show, that the wettest portions of the lake district are those situate at the head or eastern extremity of those valleys formed by the highest mountain ridges, amongst which are the Great Gabel (2925 feet above the sea), Sca Fell (3166 feet), Glaramara, Red Pike, and Honister; the first being apparently the grand central point of attraction and condensation for the warm vapour arriving in a south-westerly current across the Atlantic.

169. "The great difference in the fall between places closely contiguous to each other is very remarkable: the proportion which obtains between Ennersdale Lake, and a farm house about one and a half mile distant, is as 2 to 1 nearly.

170. "Loweswater, Buttermere, and Gatesgarth are all in the same line of valley, surrounded by the same ridges of mountains, and are each distant about two miles from the other. Buttermere exceeds Loweswater by 18 inches, or one-fourth; but Gatesgarth, at the head of the valley, exceeds Buttermere by 36·65 inches, or nearly one-half. Here the difference between the head and foot of the

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[1] The average fall of rain at Oxford is about 27 inches.

valley, in a distance of 4 or 5 miles, is 54·588 inches. But the great increase in the fall towards the head of the valley is appreciable at much more limited distances."—*See Report of British Association,* 1846.

171. At the meeting of the British Association in 1852, *A report was presented by Lieut.-Col. Sykes on the mean temperature and fall of rain at various stations in the Bengal Presidency, from official registers, for the year* 1851; which shows, in the case of Cherraponjie, the effects of elevation in producing rain, and probably the most extraordinary amount of rain ever recorded in one year. "At that station the almost incredible quantity of 610·35 inches of rain fell in 1851, and," said the Colonel, "that this deluge is no mistake of record; independent of the official report which I quote, I have a letter from Professor Oldham in confirmation of the fact, who spent the monsoon of 1851 at Cherraponjie, and kept a separate record: 50 feet 10 inches depth of water may be said to have fallen, chiefly in seven months;" the rain from November to March, both inclusive, being only five inches and three tenths. "The explanation of this fall at Cherraponjie is in the physical circumstance connected with its location. The station is on the Cossya hills, at 4500 above the sea, facing the south; and the vapour from the Bay of Bengal, floating at a height of about 4500 feet, passes over the plains of the Deltas of the Ganges and Brahmapootra, and first impinges upon the Cossya hills, and is immediately *condensed by the lower temperature of the hills.*"

172. This excess of rain on mountains (which I attribute to their conducting the electricity from the clouds) is very generally attributed to the coldness of the mountains condensing the vapour of the atmosphere, thus causing its precipitation; and both Mr. Miller and Colonel Sykes adopt this view in reference to the foregoing facts. I know of no proof of mountains being colder than the air, at a like elevation over plains, and at a distance from them. I believe the contrary to be the fact, and *that the low temperature of mountains is in a great measure the re-*

sult of atmospheric influence, and consequently that the vapour of the air cannot be condensed by them.

173. Boussingault, in the account of his ascent of Chimborazo, states, that when at an elevation of 18,634 English feet, "It was a quarter past one, and we experienced considerable cold. The thermometer had fallen to $32\frac{3}{4}°$ Fahr. We were enveloped in a cloud. The hair-hygrometer indicated 91°5; and after the cloud was dispersed it remained at 84°. Such a degree of moisture at so great a height might appear remarkable, but I have *often* remarked the same thing on the Andes, and it seems to be quite capable of explanation:—*During the day the surface of the snow is generally moist;* the rock Peña Colorada, for example, *was quite wet;* the air immediately round the glacier might therefore be saturated with moisture. On Mont Blanc Saussure saw his hygrometer stand at between 50° and 51°, while the temperature was from $32\frac{1}{8}°$ to $37\frac{1}{6}°$ Fahr."

174. When on the summit of the mountain, at two o'clock, "We were at an absolute height of 6004 metres (19,698 feet), the greatest elevation to which, I believe, man has hitherto attained on mountains. In the shade of a rock the thermometer stood at 46° Fahr. I sought, but in vain, for a corner, in which I might be able to ascertain the mean temperature of the station. One foot under the snow the thermometer stood at 32° Fahr.; but the snow was in a melting state, so that it could not afford any other result. While we were occupied with our observations on Chimborazo, we had uninterrupted fine weather, *and the sun was so hot as even to annoy us a little.*

175. "The descent was difficult. After descending 300 to 400 metres (984 to 1312 feet) we encountered a cloud. A little lower down it began to hail, and *the air was thus considerably cooled.* At the moment we again found the Indians, who took charge of our mules" (at an elevation of 16,223 feet), "the cloud poured down upon us hail of such a size, that we experienced from it pain both on our hands and faces."—*Edinburgh New Phil. Mag.*, July, 1835.

176. From the foregoing extracts, it appears that the temperatures observed were much higher than is generally assigned to such elevations; and it seems as if the coldness alluded to (173) was produced by the cloud, and not productive of it. The hail also, which was so cold as to *considerably cool* the air, could not have been produced by condensation by a mountain on which the snow was melting, and on which the thermometer indicated a temperature several degrees above the freezing point; but may have resulted from the escape of the electricity from the vapour floating around and above the mountain: and the effect of the hail on the hands and face seems to show that it must have fallen from a considerable elevation. It also appears that during the day the surface of the snow is *generally moist*, and on the summit of the mountain the sun was *unpleasantly hot*; and this, at an elevation of 3¾ miles above the level of the sea. From this it seems that the temperature on the mountain must, during the day, be considerably warmer than the air at a like elevation at a distance from it; therefore the formation of clouds at such times cannot be the effects of condensation, by the mountain, of the vapour from the colder air.

177. In the extract below it is stated, in allusion to Chimborazo, that *the formation of clouds is very frequent on the summit of snowy mountains, particularly in fine weather, and always some hours after the culmination of the sun*; consequently after the surface of the mountain has been warmed by the sun's rays. These facts tell against the theory that mountain sproduce clouds by condensation, but greatly in favour of the opinion that they do so by conducting away the electricity from the vapour; as the insulation of the vapour floating in the air around the mountain, would be far more complete with the surface of the snow frozen and dry, than when it is moist and the air damp from evaporation going on from the mountain itself.

178. "With what interest does not one contemplate the display, in so small a space, of all the chief meteorological phenomena. Here one of those immense broad clouds, which were by Saussure so well named parasitical clouds

(*Schmarotzerwolken*), begins to attach itself to the middle portion of a trachyte cone; it adheres firmly to it, and is not at all affected by the wind, however strong it may be. Soon a flash of lightning darts from the centre of this mass of vapour; hail, mixed with rain, descends on the foot of the mountain, while its snowy summit, which the storm cannot reach, is brilliantly illuminated by the sun. At a greater distance rises a summit of bright shining ice; its outline is delineated with sharpness on the blue heaven, and all the peculiarities of its form are distinctly visible. The atmosphere is of remarkable purity; nevertheless the snowy summit becomes covered with a cloud. The cloud seems to come from the interior of the mountain,—one could almost believe he sees smoke ascending; a little later this cloud is nothing more than a thin vapour, and then is entirely dissipated. But not long afterwards it again appears, merely once more to vanish. This intermitting formation of clouds is a very frequent occurrence on the summits of snowy mountains: it is to be seen more particularly in fine weather, and always some hours after the culmination of the sun."—*Ibid.* p. 89.

179. Now although Boussingault found the temperature so high on the summit of the mountain, where he says these clouds are formed, and this too at the time and under circumstances similar to those which usually produce them, still he ascribes the formation of clouds to condensation. Humboldt, during his ascent of Chimborazo, found the temperature so much higher than he expected, that he was led to think the internal heat of the mountain had an effect in elevating the temperature of its surface; and he found the temperature of the air was $34^{\circ}\cdot88$ Fahr., while a thermometer, buried three inches in the sand, stood at $42^{\circ}\cdot44$ Fahr. And yet, although the temperature was so high for the elevation, he was surrounded by mists and clouds during almost the whole time of his ascent; and while descending the mountain, and still at an elevation of $3\frac{1}{2}$ miles, "it began to hail violently. The hailstones were opaque, and milk-white, with concentric layers." To me it seems difficult to ascribe this formation

of clouds and fall of hail to condensation; and if the phenomena be so ascribable, it must be that the vapour rising from the mountain was condensed by the coldness of the atmosphere, and not the vapour of the air condensed by the low temperature of the mountain.

180. In the Reports of the British Association for the years 1846 and 1848, there are two papers given by Lieut.-Col. Sykes, F.R.S., *On the Fall of Rain, &c. on the Coast of Travancore, and the Table Land of Uttree, from Observations of Major-General Cullen, Resident in Travancore.* Two places (between 8° and 9° north latitude) are especially referred to, and the observations at those stations afford important points for consideration.

181. Observations on the temperature and the fall of rain were made at a spot called Uttree·Mullay, thirty miles E.N.E. of Trevandrum, *at an elevation of* 4600 *feet above the sea,* simultaneously with others at Trevandrum, *at an elevation of only* 130 *feet,* from the 23rd of June until the end of December. The fall of rain on the table land was 164 inches, while the fall at Trevandrum was only 36 inches. *The variation of the monthly mean temperature at Uttree Mullay was only from 64° to 67° Fahr., and at Trevandrum from 77½° to 78½° Fahr.*

	Mean Temperature.		Fall of Rain.	
	Trevandrum.	Uttree Mullay.	Trevandrum.	Uttree Mullay.
1844	°	°	inches	inches
June 23 to 30	78½	66	0¾	7¾
July	78	67	3¾	26¾
August	78¼	64	5¾	23
September	78½	66	3¾	6
October	78½	66	15¼	41½
November	77½	65	4½	36¼
December	78	64	2¼	23
Mean tem.	78½	65½	To. 36	164

182. "There appear to have been two occasions when the fall of rain at Uttree Mullay was remarkable in twenty-

four hours. On the 10th of October there fell 9 inches, and on the 26th of November there fell $7\frac{1}{3}$ inches; but there also fell from the 6th to the 10th of October inclusive, 29 inches, or more than falls in most of the counties in England in a twelvemonth. The fall of rain for the whole year was 290 inches."

183. The following is an abstract of the meteorological registrations of the temperature and the fall of rain at Uttree Mullay, and the fall of rain at Trevandrum, during the year 1846; the temperature at Uttree Mullay being recorded thrice daily, 6 A.M., 2 P.M., and 6 P.M., and the fall of rain twice daily, 6 A.M., and 6 P.M.

	Uttree Mullay.						Trevandrum.
	Thermometer.			Fall of Rain.			Rain.
	6 A.M.	2 P.M.	6 P.M.	Night *.	Day *.	Total.	
1846	°	°	°	inches	inches	inches	inches
Jan.	$57\frac{1}{2}$	$66\frac{3}{4}$	$62\frac{1}{4}$	3·050	1.600	4·650	0·100
Feb.	60	$68\frac{3}{4}$	64	0·400	0·050	0·450	
Mar.	$62\frac{3}{4}$	$71\frac{1}{4}$	$64\frac{3}{4}$	0·550	0·175	0·725	1·075
Apr.	$64\frac{1}{2}$	$73\frac{3}{4}$	67	4·050	5·550	9·600	4·025
May	$64\frac{3}{4}$	72	$66\frac{3}{4}$	17·100	19·700	36·500	11·425
June	$64\frac{1}{4}$	$66\frac{1}{4}$	$64\frac{3}{4}$	28·700	23·300	51·050	17·750
July	63	68	67	17·400	14·675	33·325	6·925
Aug.	$65\frac{3}{4}$	$68\frac{1}{4}$	$66\frac{1}{4}$	8·850	13·375	22·125	3·675
Sept.	$65\frac{3}{4}$	$70\frac{1}{4}$	$66\frac{1}{2}$	1·925	5·600	7·325	0·750
Oct.	$64\frac{1}{2}$	68	$64\frac{1}{4}$	23·750	14·200	38·250	17.500
Nov.	$63\frac{1}{2}$	$68\frac{1}{4}$	65	10·325	11·350	21·675	4·400
Dec.	60	$66\frac{3}{4}$	$63\frac{3}{4}$	7·100	3·200	10·200	2·300
Mean	63	69	$65\frac{1}{3}$	123·200	112·775	235·875	69·925
	$\underbrace{\hphantom{63 \quad 69 \quad 65}}_{\text{Mean } 65\frac{2}{3}}$						

* *The headings of the day and night columns are reversed in the Reports of the British Association; which is evidently a mistake.*

184. "The average annual fall of rain upon this elevated table-land during the day and during the night does not appear to differ materially, although it is somewhat in excess at night, being 123·2 inches to 112·7 inches during the day. The excess at night, however, does not hold good

through all the months. In May there were 17·1 inches by night and 19·7 inches by day; in August 8·8 inches by night and 13·3 inches by day; and in September 1·9 inches by night and 5·6 inches by day. While 235·8 inches fell in 1846 at Uttree Mullay, 69·9 inches only fell at Trevandrum. 7·6 inches of rain fell on the 25th of May at Uttree Mullay; and there are three instances of a daily fall of nearly 7 inches."

185. "The monthly mean temperature at Uttree Mullay, at 6 A.M., varied only from 57°·5 in January to 65°·75 in August and September; at 2 P.M. from 66°·25 in June to 73°·75 in April; and at 6 P.M. from 62°·25 in January to 67° in the months of April and July. The annual mean of the respective hours was 63°, 69°, and 65°·18, and the mean of the whole observation 65°·7. The extreme annual range of the thermometer was from 55°·5 on the 31st of January and 15th of December to 78° on the 6th, 21st, and 22nd of April, *so that the extremes differed from the mean by only* 10°·2 *minus and* 12°·3 *plus*. With such limited general results it would be superfluous to particularize monthly variations of temperature."

186. The foregoing reports show the effects of elevation in producing rain; but although Uttree Mullay is 4470 feet more elevated than Trevandrum (181), the difference in the mean temperature of the two places in 1844 was but 13°, showing a decrease of temperature of not more than 1° for every 344 feet of elevation; therefore the great precipitation of rain at the former place cannot very well be attributed to the vapour being condensed by the low temperature at that elevated station. It may also be well to remark, that the fall of rain by night is very little in excess of that by day, and in some months more rain fell by day than by night.

187. The very equable temperature at Uttree Mullay tells strongly against the theory (40) which attributes the fall of rain to a mixture of various currents of air of unequal temperatures; as it is shown that the temperature throughout the whole year is subject to very little change.

188. The excessive fall of rain at Cherraponjie (171) (and mountains in general) suggests another point for consideration. It is a generally received opinion that a great amount of heat is set free on the condensation of vapour; it may be so, but I have no opinion on the subject: however, if this is the case, the fact may be advanced as another objection tô the theory that mountains produce rain by condensing the vapour floating in the air; as from the excessive fall of rain on that mountain the temperature ought to be raised in a very great degree by the heat set free from such an enormous condensation of vapour. Then comes the question, how or why does condensation continue to go on under such circumstances? as the liberated heat ought to put a stop to all further condensation. But the fact does not tell against the theory that mountains produce rain by enabling the electricity to escape from the already condensed vapour, as the damper and warmer the air, the more readily would it conduct the electricity from all vapour floating in the neighbourhood.

189. Dr. Dalton, adopting Dr. Hutton's theory on the cause of rain, gives the following explanation on the subject: "High mountains produce rain, I think, unquestionably from their obstructing the horizontal currents of the air, and causing them to ascend into the higher regions of the atmosphere, by which airs of different temperatures mix together."—*Edinburgh New Phil. Journal*, July, 1833.

190. Assuming Dr. Hutton's theory to be correct, this explanation might be considered a fair solution of the question; there can be no doubt that much vapour may be carried upwards by currents of air so generated, but so far it is equally applicable to the theory I advance; still I believe the excessive rain on mountains cannot be satisfactorily accounted for in that way: and the fact (157) that clouds on the sides of mountains will remain at the same elevation for days together, seems to tell against the existence of such upward currents prevailing in any great degree; and that clouds do so retain their position has

been remarked by many observers, and amongst them Saussure, who, as already stated, named such clouds parasites, from their sticking so long in one place (178).

191. The fact just alluded to, of clouds seemingly remaining at one spot on the side or top of a mountain, has been explained as owing to the constant accession of fresh vapour to the cloud, thus compensating for the precipitation going on; so that, in fact, it is not continually the same cloud, but the continual formation of one.

On the Diminished Fall of Rain in elevated rain gauges.

192. Although there is an excess of rain on mountains and their immediate neighbourhood, yet it has been proved that more rain falls on the ground (at low levels) than at any height above it, the quantity received diminishing as the gauges are placed higher and higher above the earth.

193. Dr. Heberden had three rain gauges constructed precisely similar; one he placed on the roof of Westminster Abbey, another on the top of a house, considerably lower than the first, and the third on the ground in the garden; and a year's observation gave the following results:—

Gauge on the roof of the Abbey	12·099 inches
Ditto on the top of the house	18·139 —
Ditto on the ground	22·608 —

M. Arago found, from observations during twelve years, that on the terrace of the Observatory at Paris the annual fall of rain was 19·88 inches, while in the court, 71 feet lower, the annual depth was 22·21 inches. The experiments of Professor Phillips and Mr. Grey at York for twelve months (1833–34) gave the following results as the fall for the year:—

On York Minster, 212 feet above ground	14·963 inches
On the Museum, 42 ditto	19·852 —
On the ground	25·706 —

Showing that the rains increased above 70 per cent. in falling 212 feet, and at a rapidly increasing ratio.

194. This phenomenon has been explained as caused by the coldness of the rain-drop condensing the vapour on to its surface; but I cannot think this alone will account

for the very great increase, as the rain-drop would only condense and be increased by the vapour on which it might fall. The theory I propose will assist this explanation, as each drop of rain in falling from a cloud must, according to the theory, not only be increased in bulk by each particle of vapour on which it might fall, but would be further increased by attracting to itself every other particle of vapour which may be floating within the sphere of its electrical attraction, and charged more or less than the falling drop with electricity.

On the Production of Cumulus Clouds, Thunder Rains, and Hail.

195. Thunder rains (as I have denominated them, p.161), and the clouds producing them, are, I believe, attributable to causes somewhat different from those producing clouds and rain of a lighter and more ordinary character; and I submit the following in explanation of the formation of cumulus clouds. Vapour rising during calm and warm weather would rise to the height in the air at which its electric coating would render it buoyant, and it is easy to conceive, that if evaporation went on very rapidly, as in sultry weather, vapour might accumulate to such a degree as to bring the air into a conducting state as *far as the mass of vapour might extend;* the distribution of electricity over the surface of the vaporous particles would therefore be equalized, but its distribution throughout the mass of vapour would be disturbed; for the more elevated, most condensed, and higher charged vapour would lose a part of its electricity, and consequently have its buoyancy diminished; while the lower portion of the vapour would be rendered more buoyant by its electric condition being increased; and these opposite tendencies, i. e. of the higher vapour to sink and the lower to rise, would bring the particles into such a degree of proximity as would render them visible, and form clouds.

196. A cloud thus formed would increase on all sides by attracting other vapour, as both the electrical attraction, and the attraction of aggregation of the vapour of the

cloud, would be increased by its being thus collected into a mass. Very dense clouds may thus be produced, highly charged with electricity, and occasionally at elevations far above that at which clouds most generally form.

197. A cloud formed as above may be of great depth; and as the density of the air in the lower portion of such a cloud would be much greater than that in its upper part, and as the electricity of the cloud would diffuse itself equally throughout the whole mass of vapour, it follows (as already stated), that the vapour below a certain level would have more electricity than sufficient to support it, and consequently would press upward; while all the vapour above that level would be deficient in buoyancy, and press downwards; and although the electrical repulsion (i. e. electrical coating) of the particle may be sufficient to prevent rain at the edges and thinnest parts of the clouds, yet the pressure at the greatest depth of the cloud may be sufficient to overcome the repulsion, and form rain.

198. Again, as the particles of vapour in the *upper* part of the cloud would be most deficient in buoyancy, independent of the pressure before alluded to, they would have a tendency to unite into drops, when, in falling through the mass of vapour below, they would increase in bulk, and form heavy rain.

199. Rain produced by the above causes may take place at much greater elevation than that caused simply by the gradual escape of the electricity of the vapour, which will account for the formation of hail: thus a cloud may be wafted from a warm to a colder region, and although the cold may be sufficient to freeze all the particles of vapour at the exterior of the cloud, the radiation of heat would be prevented from the central part, where the vapour would remain unfrozen. Rain formed in the middle or upper part of such a mass of vapour would increase in size in falling through the dense vapour of the lower part of the cloud; it would be instantly frozen on leaving the cloud, and the drop, formed under such circumstances, being large, would not only remain frozen while falling through the warmer strata of air to the earth, but would also in-

crease in size by attracting to itself other vapour; but snow or rain falling from thinner parts of the cloud, being in smaller drops, if frozen in the higher regions, would be melted in falling through the warmer air; and thus, as is often the case, there may be hail, with rain or snow falling at the same time from the same cloud.

200. The very heavy and destructive hailstorms which sometimes occur may be otherwise accounted for (242); the foregoing is offered as an explanation of such hailstorms as generally occur in early spring, and such as those described by Mr. Sturgeon (105) in reference to his kite experiments. Such storms are often accompanied by lightning; sometimes a single flash may occur, with very startling and heavy thunder; or flashes may follow in succession, but with longish intervals. To this phenomenon I will now direct attention.

201. The description I offer of the formation of cumulus clouds affords an explanation of the cause of lightning; as the electricity, being as it were pressed out of the cloud, would accumulate on the surface, and strike off either to the earth or other clouds not so highly charged.

202. It is a question with many why a cloud does not give off its whole charge at once, and why there should be a succession of flashes of lightning from the same cloud? The theory, I believe, will afford an explanation of the cause. Successive discharges of lightning are probably owing to various causes, and may be produced by the clouds being brought over some object exerting a more than ordinary attraction on the electricity of the cloud; or by a change in the form of the cloud bringing it within a striking distance of any object; or by the approach of a cloud not so highly charged; and similar causes. But I believe the principal cause of *successive flashes from the same cloud* is the agglomeration of its particles, and the formation of rain. Thus, taking as before (70), the 200,000dth of an inch as the diameter of a particle of vapour, and the 40th part of an inch as the diameter of a drop of rain, it would take 125,000,000,000 of such particles to make such a drop; but the capacity of the rain-drop for electricity, i. e.

its surface, would only equal that of 25,000,000 of the particles of vapour, or the 5000dth part of the whole; therefore on the formation and fall of such a drop of rain it would only take with it from the cloud the 5000dth part of the electricity of all the particles of which it may be formed; the remainder being dispersed through the remaining vapour, and consequently increasing the electric charge of the cloud; as the rain in falling would only take with it a charge of electricity in proportion to its extent of surface.

203. The foregoing explanation I believe will *especially* apply to lightnings from lofty and insulated clouds, and which occur during such hailstorms as are alluded to in paragraphs 105 and 199. It may also account for the dispersion of clouds after rain; for if the electricity does not, by some means, escape from the cloud in so great a proportion as the accumulation goes on through the formation of rain, the electricity must increase so as to stop the formation of rain; and may disperse the cloud altogether, through the increase of the electricity of the particles of vapour.

204. I believe that in heavy thunderstorms, and in fact at all times when the lower clouds have but a slight elevation, the electricity which passes from the clouds in lightning is but trifling when compared with that which is conducted from them by rain, damp air, &c. This subject is discussed in 211 and the following paragraphs.

205. Respecting the phenomena of thunderstorms; although I have carefully observed them for so many years, whenever an opportunity has occurred, still it has been under disadvantages, Oxford being so situate as to afford but few facilities for meteorological observations. I believe the *beginning* of the formation of a thunder-cloud to be as I have desccribed that of cumulus clouds in paragraphs 195 and 196. The formation of a cloud may be confined to a comparatively small space, or spread over a large extent of country; but the accumulation of vapour, and the violence of the resulting storm, is generally in proportion to the calmness and warmth of the weather preceding it, and

the time the weather has been in those conditions. I beg to be understood as referring to the *first* formation of the cloud, and not to the approach of a cloud already formed. A thunderstorm may be ushered in by a violent wind, or may come on with no such accompaniment; but, from all I have seen, I believe thunder-clouds *begin* to form in calm weather only [m].

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[m] "The appearance of the heavens previous to and during a thunderstorm was first diligently studied by Beccaria. He noticed that a dense cloud was first formed, increasing rapidly in magnitude, and ascending into the higher regions of the atmosphere. The lower end is black, and nearly horizontal; but the upper is finely arched, and well defined. Many of these clouds often seem piled one upon the other, all arched in the same manner; but they keep constantly uniting, swelling, and extending their arches. When such clouds rise, the firmament is usually sprinkled over with a great number of separate clouds of odd and bizarre forms, *which keep quite motionless.* When the thunder-cloud ascends, these are drawn towards it, and as they approach they become more uniform and regular in their shapes, till coming close to the thunder-cloud their limbs stretch mutually towards each other, finally coalesce, and form one uniform mass. But sometimes the thunder-cloud will swell and increase without the addition of these smaller adscititious clouds. Some of the latter appear like white fringes at the skirts of the thunder-cloud, or under the body of it; but they continually grow darker and darker as they approach it.

"When the thunder-cloud, thus augmented, has attained a great magnitude, its lower surface is often ragged, particular parts being detached towards the earth, but still connected with the rest. Sometimes the lower surfaces swell into large protuberances, tending uniformly towards the earth; but sometimes one whole side of the cloud will have an inclination to the earth, which the extremity of it will nearly touch. When the observer is under the thunder-cloud, after it is grown large and is well formed, it is seen to sink lower, and darken prodigiously; and at the same time a great number of small clouds are observed in rapid motion, driven about in irregular directions below it.

206. A cloud formed under such circumstances may be enormous in height, and of great extent; consequently the conditions I have described may prevail in an extraordinary degree. Such a mass of vapour may collect with little or no atmospheric disturbance, but the formation of the first drop of rain may begin the storm. *Such a dense cloud may be regarded as a vast mass of electricity, interspersed with minute particles of water, the former being in the proportion of not less than* 1000 *to* 1 *of the latter.* Let us consider what would be the consequences of a formation of rain in such a cloud; if but a few particles of vapour coalesce and form one drop, they would be no longer buoyant (117), and the drop in falling through the dense vapour would increase in bulk from contact with other particles; now as the electricity set free by this agglomeration of particles would instantaneously pass away, either to the surface of the cloud, or by dispersion amongst the particles composing it, a vacuum or rarefied space would result on the instant of the formation of rain, when the sudden pressure of the surrounding portion of the cloud into the space would bring more particles into contact, and more rain would be formed.

207. As the electricity, liberated by the formation of rain, could not readily be diffused throughout the cloud, owing to the close approximation of the particles of vapour,

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While these clouds are agitated with the most rapid motions, the rain generally falls in abundance; and if the agitation be very great, it hails."

Dr. Thomson makes the following remarks on the first formation of a thunder-cloud:—

"A low dense cloud begins to form in a part of the atmosphere that was previously clear. This cloud increases fast, but only from its upper part, and spreads into an arched form, appearing like a large heap of cotton wool. The under surface is level, as if it rested on a smooth plane. *The wind is hushed, and every thing appears preternaturally calm and still.*"—Noad's Manual of Electricity, par. 262, 264.

it must in a great measure pass off to the surface of the cloud, where it might accumulate, till, overcoming the resistance of the air, it would fly off as lightning; when the consequent concussion of the air would tend to produce more rain, by driving the closely packed particles of the cloud into contact. Thus the formation of rain may be the cause of lightning, and lightning may produce rain; so that the very heavy rain which accompanies or immediately follows a flash of lightning may be both a cause and effect.

208. Rains sometimes occur of such an extraordinary character, and so enormous in amount, that the accounts would hardly be believed if not resting on good authority. The following is a list of well authenticated cases, selected from various papers in the *Reports of the British Association*.

33 inches fell in	26 hours at	Gibraltar, Nov. 27, 1826.
31	22	Joyeuse, Oct. 9, 1827.
30	24	Genoa, Oct. 25, 1822.
18	$7\frac{1}{2}$	Catskil, U.S., July 26, 1819.
$14\frac{1}{2}$	18	Viviers, Sept. 6, 1801.
6	3	Geneva, May 20, 1827.
$3\frac{1}{9}$	2·17 min.	London, August 1, 1846.

In the case at Gibraltar the rain which fell in 26 hours was about equal to the average fall at Oxford in a year and a quarter; and in the storm which visited London on 1st of August, 1846, which must be remembered by many persons, the fall of rain in 2 hours and 17 minutes was equal to the average fall in 6 weeks.

209. Enormous as such rains appear, still I believe the theory proposed will afford a fair explanation of the causes producing them. Thus a cloud formed as described, that is, piled to a great height, and with its particles pressed nearly into contact, may be vast in extent; and after sultry weather the surrounding atmosphere also may be so saturated as to be as it were *a mass of invisible vapour.* Now as water is 860 times heavier than the air at the sea level, each particle of vapour, to be buoyant, must, toge-

ther with its electrical coating, occupy the space of an equal weight of air; therefore the fall of an inch of rain would produce a vacuum or rarefaction equal to that which would be produced by the abstraction of 860 inches of air: the space occupied by the vapour would, in fact, be greater than this, as vapour would occupy a greater space according to its elevation; but as the density of the air decreases with its height, the effects would be the same, that is, the vacuum or rarefaction would be equal to that which would result from the abstraction or annihilation of 645 cubic feet of air, above every square yard over which such rain might fall, and during the time it would be falling.

210. Rain from such a cloud as I have described could not be otherwise than heavy; and the heavier the rain, the more rapid and extensive would be the production of the vacuum or rarefied space, consequent on the fall of rain and escape of its electricity; into which space other portions of vapour would be impetuously driven, and consequently produce more rain. Another effect must result from such heavy rain; as, from its volume, it would readily conduct the electricity of the cloud to the earth; and the free escape of electricity from the vapour would tend to produce more rain; while the pressure of the atmosphere would drive more and more vapour into the vacuum every moment created; thus, the whole vapour of a district may be driven to and discharged within a very small space.

On Electric Discharges from Clouds during Rain.

211. Although lightning is probably *at all times* accompanied by or instantaneously followed by a heavy dash of rain, whenever it strikes from a cloud to the earth, still I believe that during *excessive* rains the electricity which passes off to the earth in the form of lightning, is trifling when compared with what is conducted from the clouds by the falling torrents of water.

212. The storm in London on the 1st of August, 1846, was tremendous as far as regards the fall of rain, and the effects of the hail. Three columns of the "Times" were

full of details of the breakage of windows and skylights, the bursting of culverts and flooding of various parts of the metropolis. It stated that "the damage at Buckingham Palace was estimated at from £1800 to £2000 in glass alone. At the houses of parliament there were seven thousand squares of glass broken; at Cubitt's manufactory, Millbank, twelve or fourteen thousand; at Burford's panorama, Leicester-square, ten thousand; and at Broadwood's pianoforte manufactory eight thousand. In Regent's-quadrant every square was broken; and in many streets scarcely a whole pane was left." The fall of rain during the storm was, I believe, the heaviest ever recorded in England in the same space of time (208). And yet, although the lightning was described as vivid, and the thunder heavy, only three cases were mentioned of injurious effects from lightning; in two of these, chimneys only were struck, and in the other a window was injured, and a woman standing by it had her arms and hands burnt by the lightning. It seemed as if the falling torrents of water were the best conductors, and all the lofty buildings escaped.

213. In various other parts of the country heavy storms occurred; in most cases the lightning was described as vivid and frequent; but wherever the rain was *very* excessive, the lightning seemed to have been much more harmless than is generally the case in severe thunderstorms. At Nottingham, where the rain was described as tremendous, the lightning did little or no damage, although spoken of as very vivid; and Mr. Low, at Highfield-house, in that county, counted from twenty to fifty flashes per minute while the storm was at its height. At Faringdon, Berks, the rain was excessive, and did considerable mischief; but I believe no damage was done by the lightning.

214. On the same day the spire of St. George's church, Leicester, was destroyed by lightning; but although the lightning is described as terrific, only one other case of injury from its effects is alluded to, and that one of but little importance. "The storm which ushered in the

catastrophe was more violent and extensive than is often experienced in this country. At two in the morning it had visited the Channel Islands; it then raged violently in the neighbourhood of London; and at five o'clock in the afternoon it reached Leicester, when it discharged itself in such sudden and copious torrents of rain, that the lower part of the town was flooded. The noise of the descending torrents *for a time* almost drowned the roar of the thunder. The air was at the same time rent and traversed by flashes and streaks of lightning, which soon became nearly incessant. "A great deal of the lightning," says one of the observers, "was forked, but more came down in the form commonly, but erroneously, called 'fire-balls.' *Immense masses of flame descended to the earth, blinding every spectator for some seconds.* The extraordinary duration of the storm was as remarkable as its violence; for from five o'clock until nine in the evening there was scarcely any abatement. It was at five minutes past eight, after one or two peals of unusual distinctness, that the church of St. George was struck. For the distance of forty feet on the eastern side, and nearly seventy on the west, the massive stonework of the spire was instantly rent asunder and laid in ruins."—*The Thunderstorm*, p. 168.

215. From the foregoing facts, and many others which I have noticed, I am led to believe that although lightning is generally most dangerous when the rain is very heavy, especially if the thunder-cloud be rather elevated; yet that during *very excessive* rains, and when the lower clouds nearly reach the earth, the danger from the lightning is comparatively little, and the electricity passes off to the earth in a wholesale manner, by the falling streams of water conducting it away imperceptibly, as metal or other conducting bodies would; although during the same time the discharges of lightning amongst the masses of vapour at greater heights may be both brilliant and very frequent. And when during such heavy rains lightning does strike to the earth, it is as it were promiscuously, the rain readily conducting the electricity in any

direction; low places being struck at such times, although close to much more lofty objects, which under other circumstances would offer the greater attraction for the stroke. In the case of the church at Leicester, it may be presumed that the "copious torrents of rain" had in some degree ceased by the time the spire was struck; as it is stated that "*for a time* they almost drowned the roar of the thunder," and the storm had then raged over three hours.

216. A violent storm occurred at Liverpool on the same first of August; the lightning in which was described as more vivid, and the thunder as more frequent and heavy than could be remembered. Here again the injury to property from lightning was but little, as the striking of a chimney only was mentioned. Six persons were named as having been struck down by lightning in various parts of the town: some of them appeared insensible for a time from its effects, but none were seriously injured. This could hardly have been the case had they been actually struck by lightning. I have reasons for believing that persons may fall from what I can only describe as the discharge of a non-luminous volume of electricity from a *very low cloud* with heavy rain; and such may have been the cause in the cases above given.

217. But it is not only during heavy rains that electricity passes from the clouds to the earth otherwise than as flashes of lightning; many facts may be advanced to prove that such is the case from most clouds, and perhaps from all in some degree. The air is seldom, if ever, so dry in these regions as to thoroughly prevent the passage of electricity; and the vast quantity which passes off from clouds is shown by its effects on electric kites, pointed rods, and the like apparatus. St. Elmo's fires, and the effects on animal sensation, may also be advanced in proof. Many cases might be given to show that the electric discharge is great from clouds at considerable elevations, but the following will suffice. Professor Forbes thus describes a phenomenon which occurred near Mont Cervin. The atmosphere was very turbid, the ground

was covered with half-melted snow, and some hail began to fall. "We were perhaps 1500 feet below the Col, or still 9000 above the sea, when I noticed a curious sound, which seemed to proceed from the Alpine pole with which I was walking. I elevated my hand above my head, and my fingers yielded a fizzing sound. There could be but one explanation—we were so near a thunder-cloud as to be highly electrified by induction. I soon perceived that all the angular stones were hissing round us, like points near a powerful electrical machine." Immediately after this it thundered. There can be hardly a doubt that had this phenomenon occurred in the night, all these angular stones would have been luminous, and that the edges and points were receiving electricity from the cloud, as elevated electric rods do during the passing of a thunderstorm; and if so, how great must have been the quantity passing from the cloud to the earth!

218. Returning to a consideration of the storm in London, it may be worthy of remark, that for some time previous to the 1st of August the weather had been extremely hot, the thermometer registering as high as from 82° to 86° in the shade; and the wind during the same time was steady from the east and north-east, consequently well calculated to keep up the insulation of the suspended vapour. The wind changed suddenly at Oxford about midday to the opposite quarter, and in London the like change occurred about three hours later, just before the storm commenced. This fact may be thought to favour Dr. Hutton's theory of rain being produced by the mixing of airs at different temperatures; I believe the storm was commenced by the moisture of the south-west and west wind destroying the insulation of the lower air.

219. To the fact that the fall of rain is productive of winds, I shall more particularly refer when on the question of the cause of storms; but the following extract will show, that during the storm in London the wind blew from all points towards the centre of the storm. "In the storm which visited the metropolis there were many remarkable features which deserve notice. Not

merely were the hailstones of an unusual size—many of them being between three and four inches in diameter—but, it is evident, from the directions in which the largest amount of damage was done to windows, &c. that the storm must have moved along a curved line from the S.E. towards W., and then towards the N.E.; having, at the same time, an internal motion, or probably, *a series of currents setting from the circumference towards the centre*, along its line of direction. In this respect it resembled, in a remarkable degree, the hurricanes of the tropics; and that it was a circular mass of vapour, passing by a line as nearly as possible in the direction above indicated, will be evident, if the different localities which suffered are examined."—*Athenæum*, Aug. 8, 1846. The general phenomena of the storm, the form of the hailstones, &c. were described as being singularly similar to those of the storm in Cheshire described by Halley.

On the Cause of Hailstorms.

220. Storms sometimes occur during which hailstones of immense size, or rather pieces of ice, have fallen. Accounts of many such are given in the *Phil. Transactions*, *Encyclopædias*, &c. Such storms are often dreadfully destructive; the most preeminent in this respect, of all European storms on record, being that which desolated so large a portion of France in July 1788; traversing nearly the whole length of the country, and destroying property of the estimated value of more than a million sterling[n]. The

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[n] "This tremendous storm was ushered in by a dreadful and almost total darkness, which suddenly overspread the whole country. In a single hour the whole face of nature was so entirely changed, that no person who had slept during the tempest could have believed himself in the same part of the world when he awoke. Instead of the smiling bloom of summer, and the rich prospects of forward autumn, which were just before spread over the face of that fertile and beautiful country, it now presented the dreary aspect of universal winter, in the most sterile and gloomy of the arctic regions. The soil was changed into

storm which occurred with such violence at Chipping Norton and its vicinity on August 9, 1843, must be painfully remembered by many. However I need not enumerate more such cases, but will give Halley's description of a storm he witnessed, as generally characteristic of these destructive storms as they occur in Europe. The only peculiarity in this case being, that it took place much earlier in the year than is usual for such storms, which generally occur in the hottest months.

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a morass, the standing corn beaten into a quagmire, the vines were broken to pieces, and their branches bruised in the same manner; the fruit trees of every kind were demolished, and the hail lay unmelted in heaps, like rocks of solid ice. Even the robust forest trees were incapable of withstanding the fury of the tempest; and a large wood of chesnut trees in particular was so much damaged, that it presented, after the storm, little more than bare and naked trunks. The vines were so miserably hacked and battered, that four years were estimated as the shortest period in which they would become again in any degree productive. Of the sixty-six parishes included in the district of Pontoise, forty-three were entirely desolated; while of the remaining twenty-three, some lost two-thirds, and others above half their harvest."

"M. Arago has lately remarked, that the damage done to a thousand and thirty-nine parishes on this occasion amounted to 24,962,000 francs."

"Leslie computes that hailstones sometimes fall with a velocity of 70 feet per second, which is at the rate of about 50 miles an hour. "Striking the ground with such impetuous force, it is easy," he says, "to conceive the extensive injury which a hail-shower may occasion in the hotter climates. The destructive power of those missiles in stripping and tearing the fruits and foliage, increases besides in a faster ratio than the momentum, and may be estimated by the square of their velocity multiplied into the mass. This fatal energy is hence as the fourth power of the diameter of the hailstone."—*Encyclopædia Metropolitana*, art. Meteorology.

Halley's Account of a Hailstorm on the 29th of April, 1696.

221. "The vapour that disposed the aqueous parts thus to congeal, came with a south-west wind out of Carnarvonshire, passing near Snowdon, with a horrid black cloud, attended with frequent lightnings and thunder. I hear no further of it westward than out of Denbighshire, where it left St. Asaph to the right, and did much damage between it and the sea, breaking all the windows on the weather side, killing poultry, lambs, and a stout dog; and in the north parts of Flintshire several people had their heads broken, and were grievously bruised in their bodies. From Flintshire it crossed over the arm of the sea that comes up to Chester, and was only felt in Cheshire, at the very north-west corner of the peninsula, called Wirrall, between the æstuaria of Chester and Liverpool, at a town called West Kirkby, where it hailed only three minutes, it being on the extreme point of it on the right hand; but it thundered dreadfully, and was here about three in the afternoon; but the main body of it fell upon Lancashire, in a right line from Ormskirk to Blackburn, on the borders of Yorkshire; the breadth of the cloud was about two miles, within which compass it did incredible damage, killing all sorts of fowl and small creatures, and scarcely leaving any whole panes in any of the windows where it passed; but, which is worse, it ploughed up the earth, and cut off the blade of the green corn, so as utterly to destroy it, the hail-stones burying themselves in the ground; and the bowling-greens, where the earth was anything soft, were quite defaced, so as to be rendered unserviceable for a time. The hailstones, some of which weighed five ounces, were of different forms, some round, some half round, some smooth, others embossed and crenulated, like the foot of a drinking glass, the ice very transparent and hard, *but a snowy kernel was in the middle of most of them, if not all;* the force of their fall showed that they fell from a great height. What I take to be the most extraordinary in this phenomenon is, that such a sort of vapour

should continue undisturbed for so long a tract as above sixty miles together, and in all the way of its passage occasion so extraordinary a coagulation and congelation of the watery clouds, as to increase the hailstones to so vast a bulk in so short a space as that of their fall."

222. Up to a very recent period it was generally stated in meteorological works, that hail very rarely occurred in tropical regions, except above certain elevations on mountains, and even there but seldom. This opinion prevailed till, at the meeting of the British Association in 1850, a communication *On Indian Hailstorms* was made *by Lieut.-Col. Sykes;* the statement being the result of researches by Dr. George Buist of Bombay, F.R.S., &c. A long list was given, from which "a few instances will suffice to show the character of these storms, *and these are given from European testimony.*"

223. "April 10, 1822, at Bangalore, a hailstorm killed many cattle, the hailstones being represented by the natives as being as large as pumpkins. The gentleman who gives the account says, *Three days after the storm* I went to the spot, and found the carcases of twenty-seven bullocks lacerated by hailstones; also dead birds. In a tank 300 yards in circumference, half the surface was covered with floating masses of hailstones, which had been carried down the ravines two days before; some of the masses were five and a half inches in thickness; the hailstones were angular and oval, and some measured three inches in diameter. This was the third day after the fall, in the scorching month of April."

224. Nineteen other storms were especially noticed, four of which were fatal to human beings, cattle, &c. In all the storms the hailstones seem to have been very large, being compared by the different observers, in various places, to the size of pullets' eggs, goose eggs, billiard balls, and cocoa-nuts. Four, six and a half, and eleven ounces, and two pounds are given *as the actual weight of single hailstones;* and two and five inches as the diameter, and eleven and a half and twelve inches in circumference, as the *real measure* of them in other places.

225. At the meeting of the British Association in 1855, another communication was read, *On Remarkable Hailstorms in India;* also by Dr. Buist. The following are extracts. "The great distinguishing characteristics of the Indian, as contrasted with the European hailstorms, is, that with us (in India), in the great majority of cases, the hail which falls exceeds the size of filberts; at home it seldom amounts to that of peas or beans. That which here is the rule, occurring many times in the year, is in Europe the exception, not happening oftener than once in ten or twenty years."

226. "Hailstorms occur in India, so far as appears from the published extracts, in the following proportions for the various months of the year:—

January	5	July	2
February	20	August	0
March	31	September	2
April	34	October	3
May	17	November	4
June	4	December	5

"It will be seen that hail *chiefly falls in our driest months,* February, March, and April, and does not seem dependent on temperature; May, which supplies seventeen hailstorms, being the hottest month in the year; the true maxima due to the season being marked by the rains, whenever these occur near the summer solstice. December and January, almost the coldest months in the year, are nearly devoid of hail. We have a few instances of hail occurring in June and July in Central India, when the rains were late in setting in, but the hailstones in those cases were always small, and the falls light in comparison to those experienced in other periods of the year. The Indian hailstorms fall in patches, and seldom last above fifteen minutes."

227. "There are four occasions on which remarkable masses of ice, of several hundred pounds in weight, are believed to have fallen in India. One near Seringapatam in the last century, said to have been the size of an elephant.

It took three days to melt. We have no further particulars about it, but there is no reason for doubting the fact."

228. "In 1826 a mass of ice, nearly a cubic yard in size, fell in Khandeish."

229. "In April, 1838, a mass of hailstones, twenty feet in its larger diameter, fell at Dharwar."

230. "On the 22nd of May, after a violent hailstorm, 80 miles south of Bangalore, an immense block of ice, consisting of hailstones cemented together, was found in a dry well."

231. "These masses of ice, like many of those considered hailstones of the largest size, have, in all probability, been formed by violent whirlwinds or eddies, and seem to have reached the monstrous dimensions in which we find them, either on their approach to or their impingement on the ground; and the same thing will apply to those of much more moderate bulk, and which are commonly considered hailstones, though when examined they turn out to be a number of these aggregated together."

232. "May 22, 1851, at Chickanallenhully, lat. 12° 57′, long. 77° 38′, heavy fall of rain with thunder, lightning, and hail. Hailstones for the most part about the size of oranges and limes, which broke the tiles on the roofs of houses, &c. The next morning many hailstones as large as pumpkins were found on the plain; and one immense block, measuring four and a half feet in length, three feet in breadth, and eighteen inches in thickness, was found in a dry well."

233. "March 19, 1852, at Ootacamund, lat. 11° 50′ N., long. 76° 45′, altitude 7300 feet. Hailstones not large, but sufficient to do much damage to gardens. Lasted half an hour, when the ground was as white as if snow had fallen. *Buckets full, caught from the house-tops, were next morning large lumps of ice.*"

234. "March 19, 1853, at Nursingpore, lat. 22° 56′ N., long. 79° 18′ E., altitude 1900 feet. Fall of hail of the size of ordinary grapes, with lightning and loud bursts of thunder: and on the following day, at 2h. 10m. P.M., a similar phenomenon during bright sunshine. No cloud

in this instance was to be discerned whence the hail proceeded. No lightning or thunder accompanied this last fall of hail here; and the only body of cloud was at an altitude of 40° in the south-west quarter. The zenith was quite clear."

235. "Dec. 11, 1854, at Poorundhur, lat. 18°. 42′ N., long. 14° 12′ E., altitude 3500 feet. Numbers of persons were severely injured by the falling of large ice-flakes, many of them weighing several pounds; and cattle in considerable numbers have died from the effects of the storm."

236. "May 12, 1853, in the Himalayas, north of the Peshawur, when eighty-four human beings and three thousand oxen were killed. Of the Peshawur storm we have few details beyond the fact that the ice masses were very hard, compact, and spherical, many of them measuring three and a quarter inches in diameter, or nearly a foot in circumference; and this fact seems to have been given from measurement, not by guess."

237. "April 21, 1855, at Futtehgurh, lat. 26° 10′ N., long. 75° 10′ E. The hailstones larger than turkey eggs, and sufficient to have knocked a bullock down: and on the 24th of the same month very large hailstones, one measured seven inches in circumference."

238. "May 11, 1855, at Naine Tal, in the lower Himalayas, lat. 29° 20′, long. 79° 80′. A small preliminary shower of rain fell, deep-toned thunder rolled and reverberated, and vivid lightning streamed and blazed over the devoted station. The hail was ushered in by a few bright lens-shaped stones, as large as pigeons' eggs; then came more. Many were the weighings and measurings of these monsters over all parts of the station. Some weighed six, others eight, others ten ounces; and one or two more than a pound and a half avoirdupois, with circumferences ranging from nine to thirteen inches. Though no bullocks were killed, a monkey was, and three human beings were knocked down. Birds were killed, trees barked, and houses unroofed."

239. The foregoing extracts show the frequency and

destructive character of Indian hailstorms, and are therefore corrective of a very general erroneous opinion; they also exhibit some phenomena of the storm in a magnified degree to what is shown in European storms, and some phenomena of a peculiar character to which it may be well to refer.

240. The remarks in paragraph 231, respecting the masses of ice which sometimes fall, or are said to fall during storms, are so conclusive, that I think all such masses may be put out of the question, whether described as falling in India or Europe, and considered as agglomerations of hailstones, either before or after reaching the earth. It is not surprising that such masses, when found in India, should be attributed to the effects of the coldness of the higher atmosphere, rather than to freezing when on the ground; but the latter seems certainly to have been the case at Chickanallenhully (232). And at Ootacamund (233) the buckets-full of hail which were collected, were frozen the next morning into large lumps of ice: and if such was the case in India, the like may certainly occur in more temperate regions[o]. I was at Chipping Norton soon after the storm of 1843, when a respectable person informed me, and was positive in his assertion, that *two days* after the storm he found hailstones of six inches diameter; these must have been masses which had become frozen after falling, as the Rev. J. Jordan, in his published account of the storm, gives two inches as the diameter of the largest hailstones he saw at the time of the storm, although he mentions that some were said to have had a circumference of eight inches.

241. There can be no doubt that hailstones have often fallen of three or four inches in diameter, and of six, eight, or ten ounces in weight; such hailstones vary from soft lumps like snow-balls, to masses almost as hard as solid ice; and I will endeavour to show that a probable cause

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[o] Ootacamund is at a considerable elevation, but not sufficient for frost in April in a general way.

may be assigned for their production on the theory proposed.

242. The explanation I offer respecting the ordinary hailstorms, or hailshowers, of spring, may be sufficient to account for the lighter kind of storms in India; as from the generally greater height of clouds in tropical regions, when compared with those of England, taking into consideration the fact that rains increase while falling from the clouds to the earth, circumstances which in England would produce hailstones of the size of *peas* or *beans*, may make them as large as *filberts* in India. But the more destructive hailstorms of all countries, I believe, are caused by the sudden equalization of the electricity of masses of vapour, floating at very different heights in the air, and brought by currents or various circumstances the one over the other; or, in other words, by the sudden discharge of the surcharge of electricity from clouds at great heights, owing to the clouds at lower altitudes taking such a position as to conduct the electricity from the higher clouds to the earth, or to clouds near the earth.

243. As before stated, clouds have been seen by Gay Lussac, Humboldt, and others, at altitudes of, certainly, not less than five miles; and Boussingault was in a hailstorm on Chimborazo when at an elevation of three and a half miles, in which the "cloud poured down hail of such size as to produce pain both on hands and faces." Now it is evident that Gay Lussac would not have made his balloon ascent in bad weather, nor would Boussingault and others have chosen other than fine days for ascending Chimborazo or other mountains: therefore, if clouds were seen by them at such heights, under such circumstances, it is obvious that, at times, clouds may accumulate at such heights in a much greater degree; and also that clouds may form at every, or any height, from thence to the earth. Such being the case, it is easy to conceive, not only the possibility, but the great probability, that under favourable circumstances for producing them, and with such various currents as sometimes prevail in the air, clouds may occasionally be so arranged as to form a

series of conductors above the earth, and thus afford an escape for the surcharge of electricity from clouds floating at more than ordinary heights.

244. I have already (160) assigned a cause for the ascent of vapour to the higher regions of clouds, and nothing is so necessary for the ascent of such vapour as hot weather, with a certain degree of dryness of the lower air, so as to keep up the insulation of the vapour and clouds. Such seems to be the condition of the air in India during the seasons in which hailstorms prevail (226), as it is shown that hail falls chiefly in the driest months; and hail*storms* in Europe seldom occur, except after dry and hot weather of at least a few days' continuance.

245. It may be well to consider what would be the consequences of such an arrangement of the lower clouds as would conduct the electricity from clouds at great elevations. Particles of vapour, to float at four miles high, must occupy double the space which would be sufficient to render them buoyant at one mile high; therefore, the higher vapour must be charged with electricity in the degree necessary to its elevation; consequently any conditions arising which would permit an equalization of the electricity of clouds at such different elevations, would cause such a discharge of electricity from the higher to the lower clouds or the earth, as will account for the vivid lightning and heavy thunder which sometimes accompany hailstorms; and as on the escape of their electric coatings the particles of vapour would no longer be buoyant, precipitation would take place from heights far, very far, above that from which rain generally falls; and bearing in mind how much rain increases in volume while falling through the air, it may readily be conceived, that particles of vapour descending from such heights through dense clouds, and being extremely cold, would condense and accumulate on themselves other vapour sufficient to produce such hailstones as those described in the various hailstorms which have been alluded to.

246. Another effect of the sudden discharge of electricity from bodies of vapour is, that the vacuum or rare-

faciton of the air, consequent on the escape of the electricity, would be in proportion to the more or less charged condition of the vapour; therefore the effect would be great in the higher clouds; and although the density of the air is less at such heights than lower, still the pressure of the air into the rarefied space would be sufficient to drive the particles into aggregates; and this seems to be the first form the hailstones assume; as in the majority of cases, the centres are described as composed of particles of fine snow, or "of fine grains of hoar-frost, opaque, fibrous, and surrounded by layers of transparent ice, so distinct, that in some cases they could be counted."

247. The degree of cold necessary for the production of hailstones does not seem very difficult to account for on this theory, as at four or five miles high the temperature is very many degrees below the freezing point; and this intensity of cold may be increased by the rarefaction of the air at the moment of the passing away of the electricity of the vapour. It is true that the particles of vapour or nuclei of hailstones would at once begin to fall on such loss of electricity; but it may be borne in mind that the hailstone is not frozen through in the mass, but in minute particles on their condensation by and agglomeration to the first intensely cold aggregation or nucleus. Another point may also be considered; the velocity of the descent of the hailstone would increase during its course, consequently it would be much longer in passing through the freezing air in the first half of its descent, than through the warmer air of the lower half.

248. Eddies or whirlwinds may sometimes result from the sudden rarefaction of the air, from the causes assigned: in such cases, hailstones may be retarded in their fall, or even whirled about, so as to acquire a size far beyond the ordinary limits of such productions.

249. It is stated in many meteorological works, that clouds from which hail falls are generally low; in the *Encyclopædia Metropolitana*, "the lowness of the clouds from which hail generally results" is especially alluded to. Now I believe that hail never results from a low cloud

only. The hail-*showers* of spring are almost invariably from rather elevated clouds; and in hail*storms*, although the under surface of a cloud from which hail falls may be low, it must be piled, so that its upper portions have a very considerable elevation, or it must be electrically connected with clouds at such altitudes.

250. Many cases might be advanced, to show that hail, at times, is produced by clouds being brought the one over another by differing currents of air. I give a few in illustration of the subject. "A fine example, illustrating the junction of clouds, and of the resulting phenomena of rain and hail, was observed at Tarragona on the 15th of September, 1828. Some large clouds were seen to advance with a south-east wind, at seven A.M., discharging torrents of rain. Another great mass of cloud, driven by a westerly wind, met the former with great violence, producing thunder. As soon as the rain had ceased, an immense abundance of hail descended, at first very small, but sensibly increasing to a very great size."

251. In the above case (from the *Encyclopædia Metropolitana*) the clouds are described as meeting, but it is evident they were carried by cross currents of air, and therefore one cloud must have been higher than the other. I submit the following as a fair explanation of the phenomenon. Rain was falling from the lowermost cloud previous to their meeting; when, from the equalization of the electricity of the clouds, lightning resulted, but from the increase of the electricity of the lower cloud the rain ceased, while the particles of vapour in the higher cloud, losing electricity, were no longer buoyant at such heights; consequently in falling from so cold a region they formed hail, increasing in size as the hail, falling from the greater height, reached the ground.

252. "Another example also occurred in North America about noon on the 4th of June, 1814. A dark cloud appeared in the south-west, exhibiting an electrical appearance; some light clouds moving at the same time from the north-east, and apparently meeting the former. The united masses, after their junction, seemed to rise, and at

length to attain an extraordinary height; their appearance inducing Dr. Crookshank, the observer, to predict hail, and which presently fell in masses from thirteen to fifteen inches in circumference. Copious rain succeeded their descent."

253. In this case (also from the *Encyclopædia Metropolitana*) the upper clouds were light, and their seeming to rise, after their junction with the lower cloud, may have been from their together forming conductors for the electricity from more elevated vapour; and the *seeming* to rise may have been owing to the sinking of the higher vapour from the loss of its electricity, and thus forming clouds; the hail resulting from its precipitation. It may be worthy of remark, that in this case the upper clouds being small in volume, did not prevent the fall of rain from the lower and denser cloud.

254. In the *Edinburgh New Philosophical Journal,* Oct., 1836, an article is inserted, *On the Formation of Hail,* by M. de la Rive. It is unnecessary for me to say that it contains much valuable information; but the part to which I more particularly desire to direct attention is an account, by M. Lacoq, of a hailstorm which occurred at Clermont in the year 1835. The following extracts are taken from it.

255. "On the 28th of July the sun rose from an azure sky, no cloud appeared on the horizon, no vapour floated in the atmosphere, so that a beautiful day was anticipated. At ten A.M. the heat became intense, and at midday it was almost intolerable, and then some thin flakes of vapour floated in the air at a great distance: the wind was north, but so feeble that it in no degree tempered the heat. At one o'clock the wind increased; the white and floating clouds had descended considerably, and half an hour later covered a great part of the horizon: they had a greyish tint, which became darker and darker, till they were nearly quite black. At two o'clock they formed an immense covering over the whole of Auvergne; and it was then easy to anticipate that a frightful storm was at hand. We waited with anxiety for the issue of that majestic and terrible scene

which was preparing. Silence and consternation everywhere reigned, speedily flashes of lightning illuminated the massive vapours which covered the old volcanos of Auvergne, while the sun still shone upon a portion of La Limagne."

256. "We then heard a distant and low-muttering sound which resembled a kind of rolling, and almost about the same time we saw a vast cloud advance from the west to the east, pure white in some places, but principally on its edges, and of a deep grey colour in the centre; it approached with great rapidity, and seemed to be hurried forward by a violent west wind, which we had not previously felt at Clermont."

257. "*This cloud was evidently underneath all the others;* its borders were festooned and deeply slashed, and protuberances, in the shape of long nipples, were suspended from the lower portion. At a quarter past two, the anterior part of this cloud had approached very near to Clermont, and the noise which we had long indistinctly heard was now very intense; and I then very clearly distinguished *a very rapid motion in the edges of the cloud;* these edges seemed to me to be undulating, but in the position in which I was, what appeared to be undulations must have been the product of a very violent agitation. I then imagined that I could distinctly perceive hailstones in the edges of the cloud, and I predicted to some persons who were with me the immediate descent of hail."

258. "Accordingly, two minutes after having seen this whirlwind kind of motion, there was a fall of hailstones, which instantly broke all the tiles of the houses and all the panes of glass exposed to the north and west; for the hailstones being at the same time propelled along both by the north and the west wind, necessarily took the mean direction."

259. "The first hailstones which fell succeeded each other very slowly, then all at once their number increased so rapidly, that in ten minutes the soil was covered with them; some drops of water escaped at the same time from

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the electrical cloud, and then the distant rolling sound which we had so long heard entirely ceased; and the cloud, freed from its swelling appendages, was carried away by the wind. After some hours the sun illuminated, with its pale and feeble light, that scene of desolation which night was speedily about to envelope."

260. "It is not necessary that I should describe in detail the terrible effects of these hailstones. Suffice it to say, that some branches of trees two inches in diameter were cut asunder by them; some polished stones, which formed part of the cornice of houses, were broken on their edges; and some phonolite slabs, which were employed instead of the tiles which cover the roofs, were broken by the shock of the masses of ice."

261. After describing the force, &c. of the hailstones, the district over which the ravages of the storm extended, and other particulars, M. Lecoq continues: "In all the more ample accounts I have obtained, it is stated that the colour of the cloud was grey and white; *that its edges revolved*, that it extended from west to east, and with great rapidity, under the enormous cloud which hid the heavens from every eye. The wind also was everywhere the same, that is to say, there were two currents, the one placed over the other, which crossed each other at right angles." "The hail was everywhere of short duration; it seldom lasted for half an hour, and almost everywhere it was followed by rain, which, however, was not very copious."

262. "It happened that on the 2nd of August" (from the top of the Puy-du-Dome, before noon) "I was a witness, so to speak, of the formation of the storm, and of the congelation of the hailstones. The west wind, which had prevailed all the morning, speedily brought along with it some low clouds, which passed a few yards above my head; but the sun again appeared. I then saw other clouds detach themselves from the Mont Dore, and approach very near me, impelled by a very violent south wind, but which I did not feel till near one o'clock. When I thus saw great clouds proceeding in different directions, I could not for an instant doubt the formation of hail, and

my hopes were soon changed to reality. *So long as the two strata of clouds were not superimposed on each other, there was no appearance of hail.*"

263. "I perceived the hail in the distance precipitate itself from the lower clouds and fall to the earth; I saw it distinctly at the distance of fifty yards from the summit of Puy-du-Dome, and before my face. The cloud whence it escaped had indented edges, *and exhibited in those edges a whirling kind of movement* which it is difficult to describe: it seemed as if each hailstone was forced forward by an electric repulsion."

264. The whole phenomena detailed in the foregoing extracts fairly agree with the causes assigned; as in these cases there is a like superimposed cloud from which the lower cloud would conduct the electricity, and as the lower cloud was little in comparison to the one above, it must have been constantly surcharged from the electricity passing from the upper cloud, and therefore but little rain fell from it.

265. M. Lecoq particularly alludes to the revolving or whirling of the edges of the lower cloud. I think this may be caused by the upward rush of air into the rarefied space in the cloud, and that the peculiar noise often noticed as a storm comes on may chiefly be owing to the same cause.

266. It is noticed (234) that hail fell at Nursingpore in the middle of the day, and when no cloud could be perceived from whence the hail had fallen. I have several times seen a similar phenomenon, i.e. a fall of very large drops of rain at a time when not the *slightest* sign of a cloud could be seen in the zenith. This phenomenon may arise from a lateral discharge of the electricity from vapour floating high in the air over head (where a slight cloud may be invisible from the earth), when losing its support it must descend, and in falling through the air may become increased in bulk from the accession of other vapour.

267. That such a lateral discharge of electricity may occur, I think cannot be doubted; vapour seems to accumulate in strata, as shown by the formation of clouds at

various heights; and a particularly humid stratum, if of great extent, may have its surcharge of electricity discharged at a considerable distance from any particular point in it, and probably rain often occurs from the electricity of a cloud being discharged from a fall of rain at a distance, or some such cause.

On the Cause of Barometrical Fluctuations.

268. As in the following pages I shall have to refer to the effects of atmospheric disturbances on the barometer, I submit the following in explanation of the cause of barometrical fluctuations as connected with the fall of rain. Changes in atmospheric pressure undoubtedly are produced by various causes, such as change of temperature, high winds, and probably the ascent of vapour; but I believe the principal cause of the frequent sinking of the barometer, previous to and during rainy weather, is rarefaction of the air produced by the fall of rain and the escape of its electricity. As a rarefaction of the air must (according to the theory) result from the escape of electricity from invisible vapour, thus bringing it more into the condition to form rain; and the fall of rain must produce a rarefaction from causes already assigned. The rarefaction produced by the escape of electricity from vapour may be continuous for a time, for although the passage of electricity in motion is so instantaneous, its escape from the vapour may be very gradual.

269. The barometer often rises previous to thunderstorms; this I believe may be owing to an increase of pressure from the rapid increase of vapour at such times, and I have already (197) endeavoured to show that thunder-rains *commence* from an accumulation of vapour, and not from its gradually losing its electricity. But when a thunder-cloud is over head, and rain is falling freely, the barometer sinks considerably, except the clouds are at a more than ordinary elevation; in such cases the barometer may be but little affected.

270. The barometer sometimes rises on the *first* approach of a snowstorm. This may be caused by the

length of time the snow is falling; as the escape of the electricity, to which the falling of the snow is owing, may have been before the snow reached the zenith; consequently the rarefaction in the air would be filled up, and the snow would add to the atmospheric pressure while falling through the air.

271. It is with hesitation I offer an explanation of the cause of the very slight fluctuations which generally occur in the barometer in tropical regions; but I submit that the rains there may be of the character of thunder rains, therefore the foregoing paragraph (269) will apply; the generally greater elevation of the clouds may have an effect; it may be owing to the constant rising of the heated atmosphere in equatorial regions; or to the greater warmth, and consequent reduction of the specific gravity of the air; I believe that all these may be fairly assigned as causes, and either singly or conjunctly may be the cause of the phenomenon. But in great hurricanes the barometer has been known to sink very low.

On the Primary Cause of Aerial Currents.

272. There can be no doubt that currents are produced in the air by various causes. The effect of the sun and changes of temperature is shown by the trade-winds, land and sea breezes, &c.; but changes in the temperature of the air are very gradual, and the heated air near the earth's surface is generally driven away steadily by the colder and heavier air; the trade-winds, and land and sea breezes being amongst the most constant and regular of nature's operations. Violent winds are often produced in mountainous districts by the sudden descent of cold and heavy air into the heated valleys; and great atmospheric disturbances may occur from a sinking of the *upper* trade-winds. But I believe the principal cause of aerial currents of all kinds in temperate regions, and of hurricanes, waterspouts, and all violent winds of tropical regions, is the rarefaction of the air resulting from the fall of rain and the escape of its electricity.

On the Cause of Waterspouts and Tornadoes.

273. It has never been my fortune to see a waterspout; but from the descriptions I have met with, I believe that all the phenomena of waterspouts and tornadoes may be fairly accounted for in accordance with the proposed theory.

274. I have often, while watching a thunderstorm, when the lower surface of the cloud has been but moderately elevated, and apparently very dense, observed a sort of bag or nipple protruded from its lower surface towards the earth; and when such has been the case, no lightning has seemed to strike from the cloud to the earth, although during the time lightning has been very frequent in the clouds themselves; and from the clouds to the earth, both before the appearance of this hanging cloud and after it has again disappeared. It has always seemed to me as if the electricity from the cloud must be passing off from the protruded portion in volumes to the earth, although I have never seen any luminous appearance from it. I cannot say that I have ever seen these protuberant clouds assume the form of what I take a waterspout to be, still I believe that a like condition of cloud over water, might, in a slight degree, have produced a phenomenon of that kind.

275. I have already shown (209), that on the fall of an inch of rain a vacuum or rarefaction would result in the space above, equal to that which would be produced by the abstraction or annihilation of 645 cubic feet of air over every square yard where such rain might fall, and during the time in which it was falling. I believe waterspouts are always produced from dense cumulus clouds, and generally accompanied by or immediately followed by very heavy rain. It may often be seen during thunder weather that dense clouds will, *apparently*, attract each other, and on their approximation portions of each will stretch forward and unite (see note to 205). The like seems to be the case in the formation of a waterspout; a portion of the cloud is attracted towards the earth, and

thus forms a conductor for the *accumulated* electricity of the cloud to the earth; then, as the passage of electricity is so instantaneous, an enormous vacuum or rarefaction would be produced within the cloud on the instant of the passing off of the electricity; the water or other matter beneath the spout would have a tendency to rise into the rarefied space, and the inward and upward rush of air into the cloud would carry up spray or whatever may be within its vortex, in proportion to the intensity of its upward force: the rising air assuming more or less the character of a whirlwind, or rushing upwards in sweeping currents from all points towards the centre of the spout or tornado.

276. The great diminution of atmospheric pressure within the whirl is shown by the fact, that in violent tornadoes or waterspouts the windows, doors, &c. of buildings near the centre of the line of the tornado, are very often burst outwards, as if from the expansion of the air within the building on the sudden cessation of external pressure; even the cellar floors of buildings have been burst upward during such storms, where it has been impossible for the wind to get beneath them to force them up.

277. The connection of electricity with waterspouts and tornadoes is shown by various facts. Lightning generally ceases so long as a communication is complete between the clouds and the earth, or it only continues amongst the superior clouds: sometimes a continual blaze of lightning has been observed in the cloud over the whirl or spout, the thunder has been one continued roar, and trees within the whirl have been scorched, as if from a stroke of lightning.

278. The foregoing is, I believe, a correct description of the general phenomena of waterspouts and tornadoes, and the theory proposed seems to me fairly to account for the production of them. I will now give a few published accounts of various cases as described by observers; and as these accounts have been drawn up without reference to any theory, or else in support of other views, the facts

stated may be received as stronger proofs of the theory, where they support it, than if written by an advocate for it. But I would observe, that whirlwinds or tornadoes may occur with very little or no rain, and these may arise from like causes to those given, that is, the sudden and intense rarefaction of a space from the escape of electricity from clouds; as the intervention of a slight column of vapour might conduct electricity from lofty and consequently highly charged clouds, and produce very little rain, but merely a sinking of the cloud to a lower level.

279. I may here observe that the connection of electricity with the cause of waterspouts has long been argued by many, but I am not aware that any one has done so on like grounds to those I take on the question.

280. Several interesting accounts of waterspouts, storms, &c. are given in "*The Tempest,*" *published by the Society for Promoting Christian Knowledge*, 1848. In a description of a waterspout, as seen by Capt. Beechey near the equator, it is stated that the tube or column formed and descended about half way from the cloud to the surface of the water, "the sea having hitherto been undisturbed; but it now became agitated, and almost immediately became whirled in the air with a rapid gyration, and formed a vast basin, from the centre of which the gradually lengthening column seemed to drink up fresh supplies of water.—The column had extended about two-thirds of the way towards the sea, and nearly connected itself with the basin, when a heavy shower of rain fell from the right of the arch of the cloud, at a short distance from the spout, and shortly after another fell from the opposite side. This discharge seemed to have an effect upon the waterspout, which now began to retire.—This phenomenon was unaccompanied by thunder or lightning, although the shower of rain which fell so suddenly seemed to be occasioned by an electrical disturbance."

281. *Facts and Observations respecting a Tornado which occurred at New Brunswick, New Jersey, U. S.*, in June 1835. "The tornado was formed about seven and a half miles west of New Brunswick; and, moving at the rate of

about twenty-five or thirty miles an hour, terminated suddenly at Amboy, about seventeen miles from the place of its commencement. It appeared like an inverted cone, of which the base was in the clouds, and the vertex upon the earth. It prostrated or carried off every movable body within its path, which was from two to four hundred yards wide. Houses were unroofed, and in some instances unfloored; in others, the walls were thrown down outwards, as if burst by an explosion. There were two facts stated by Mr. Espy, and confirmed by Professor Bache, which demonstrate fully the existence of an hiatus. In a house which was exposed to the vertical influence of the tornado, a sheet was lifted from a bed, and carried into a fissure made in the southern wall, which subsequently closed and retained it. The same result was observed in the case of a handkerchief, similarly fastened into a fissure in the northern wall. Joints and rafters were torn from a house and thrown down at a distance from it of about four hundred yards. Of course light bodies were carried much further. There was no general rain, but hail and rain accompanied the fall of the other bodies. The tornado lasted, in any one place, for but a few seconds: the whole of the damage done at a farm having been accomplished, as the farmer stated, while he was passing from the front to the back of his mansion; so that by the time that he had reached the back door, there was a perfect calm. Meanwhile, his house and barn were unroofed, and all the neighbouring trees thrown down. The noise which accompanied the phenomenon was by every witness described as terrific, being best exemplified by the rumbling of an immense number of heavy carriages. Some thunder and lightning attended the tornado."

282. Dr. Hare, on observing the effects of the tornado, noticed the "partial withering of the foliage of the small shrubs, which had bent to the storm and were left growing. Each leaf was only partially withered, and he surmised that this was from the effects of the electricity associated with the tornado. *There were, on the same day, two other tornadoes about seventeen miles apart; and of which*

the nearest was about the same distance from New Brunswick."—Annals of Electricity, vol. ii. p. 200.

283. *An Account of a Tornado which passed over Providence, and the Village of Sumerset, U. S. in August*, 1838. *From a Letter by Zachariah Allen, esq., of Providence.* "It was three o'clock, P.M., during a violent shower, that I observed a peculiarly black cloud to form in the midst of light, fleecy clouds, and to assume a portentous appearance in the heavens, having a long, dark, tapering cone of vapour extending from it to the surface of the earth. The form of this black cloud, and of the cone of vapour depending from it, so nearly resembled the engraved pictures of waterspouts above the ocean, which I had freequently seen, that I should have come speedily to the conclusion that one of these waterspouts was approaching, had I not been aware that this phenomenon occupied a space in the heavens directly over a dry plain of land. Whilst attentively watching the progress of the cloud, with its portentous dark cone trailing its point in contact with the surface of the earth, I noticed several black specks, resembling flocks of blackbirds on the wing, diverging from the under surface of the clouds, at a great elevation in the air, and falling to the ground. Among these were some objects of a larger size, which I could discern to be fragments of boards, sailing obliquely in their descent. This alarming indication left no room for doubt that a violent tornado was fast approaching, and that these distant, dark specks were fragments of shingles and boards uplifted high in the air, and left to fall from the outer edge of the black conical cloud. This fearful appearance was repeatedly exhibited, as often as the tornado passed over buildings.

284. "The whirlwind soon swept towards an extensive range of buildings, within a few yards of me, the roof of which appeared to open at the top, and to be uplifted for a moment. The whole fabric then sunk into a confused mass of moving rubbish, and became indistinctly visible amid the cloud that overspread it, as with a mantle of mist.

285. "The destructive force of the tornado now became not only apparent to the eye, but also fearfully terrific from the deafening crash of breaking boards and timbers, startling the amazed spectator in alarm for his personal safety, amid the roar of the whirlwind, and the shattered fragments flying like deadly missiles near him. At one instant, when the point of the dark cone of cloud passed over the prostrate wreck of the building, the fragments seemed to be upheaved, as if by the explosion of gunpowder; and I actually became intensely excited with the fear that the moving mass might direct its march towards the open area of the yard, to which I had resorted, after abandoning a building in which I had previously taken shelter.

286. "The most interesting appearance was exhibited when the tornado left the shore, and struck the surface of the adjacent river. Being within a few yards of this spot, I had an opportunity of accurately noting the effects produced on the surface of the water. The circle formed by the tornado on the foaming water was about three hundred feet in diameter. Within this circle the water appeared to be in commotion, like that in a huge boiling caldron; and misty vapours, resembling steam, rapidly arose from the surface, and entering the whirling vortex, at times veiled from sight the centre of the circle, and the lower extremity of the overhanging cone of dark vapour. Amid all the agitation of the water and the air about it, this cone continued unbroken, although it swerved and swung around, with a movement resembling that of the trunk of an elephant whilst that animal is in the act of depressing it to the ground to pick up some minute object. In truth, the tapering form, as well as the vibrating movements of the extremity of this cone of vapour, bore a striking resemblance to those of the trunk of that great animal.

287. "Whilst passing off over the water, a distant view of the cloud might have induced the spectator to compare its form to that of a huge umbrella suspended in the heavens, with the column of vapour representing the

handle, descending and dipping into the foam of the billows. The waves heaved and swelled whenever the point of this cone passed over them, apparently as if some magical spell were acting upon them by the effect of enchantment. *Twice I noticed a gleam of lightning, or of electric fluid, to dart through the column of vapour, which served as a conductor for it to ascend from the water to the cloud. After the flash the foam of the water seemed immediately to diminish for a moment, as if the discharge of the electric fluid had served to calm the excitement on its agitated surface.* The progress of the tornado was nearly in a straight line, following the direction of the wind, with a velocity of perhaps eight or ten miles per hour.

288. "Near as I was to the exterior edge of the circle of the tornado, I felt no extraordinary gust of wind; but noticed that the breeze continued to blow uninterruptedly from the same quarter from which it prevailed before the tornado occurred. I also particularly observed that there was no perceptible increase of temperature of the air adjacent to the edge of the whirlwind, which might have caused an ascending current by a rarefaction of a portion of the atmosphere. After passing over the sheet of water, and gaining the shore, I observed the shingles and fragments of a barn to be elevated and dispersed high in air; and the dark cloud continued to maintain the same appearance which it first presented, until it passed away beyond the scope of a distinct vision of its misty outlines." —*American Journal of Science*, vol. xxxviii. p. 74.

289. The foregoing account is not only interesting, but seems to have been written by an eyewitness well qualified to describe the phenomenon, and who observed it under very favourable circumstances. The facts stated as to the effects of the flashes of electricity (for it is hardly described as lightning) as the tornado passed the river, are confirmatory of the connection of electricity with such phenomena: but if the lightning or electric fluid passed, as stated, from the water to the clouds, it at once upsets the whole theory I propose; however, this is a point on which the eye may be deceived, especially at a moment of such excitement,

even if the question can be decided by the eye alone. To this part of the question I shall again refer.

290. *The following account of the ravages committed by a waterspout at Cette, extracted from a French paper, appeared in the "Times" of Oct.* 30, 1844. "A frightful misfortune has this afternoon plunged our whole population into a state of consternation and despair. About four o'clock an electric waterspout fell upon our town, and committed such ravages, that, at the present moment, it might be supposed that the town had been submitted to all the horrors of a siege. This terrible phenomenon, which arrived in the direction of the fortress of St. Pierre, skirted the mole in its whole length; and when it came opposite the engineers' establishment, attracted probably by the conductor and the zinc roof of the house, it turned round the edifice, and at last fell with violence upon it. At the same moment a violent explosion was heard, and the whole population thought that its last hour was come. During two minutes' space of time a terrific crash resounded in the air. The roofs of the houses were smashed to pieces, and the fragments were carried to the most distant parts of the town. The building belonging to the engineers has been entirely sacked; its zinc roof was carried off in the twinkling of an eye, and the whole façade demolished and razed to the ground, so that nothing now remains of it but the back and side walls. Another house, four stories high, new, and solidly built, was literally crushed to the earth. In every apartment the separation walls were destroyed, and the windows torn out; everywhere destructive traces have been left. A fearful inundation joined at the same time its ravages to those of the electric waterspout. In an instant the water of the canal rose and flooded the quays. At least a dozen boats were sunk in the canal itself, and many persons perished. Five or six large vessels have been completely wrecked, and remain with their keels uppermost. In the streets and on the quays are everywhere to be seen wounded wretches, some with bloody heads, others with mutilated limbs. It is impossible to give any description of the feeling of terror produced

in the minds of the whole population." "The effects here described are considered by Peltier as completely inexplicable on the theory of whirls produced by the meeting of contrary winds. Arago has also admitted that they cannot be understood without the aid of Electricity."—*Noad's Manual of Electricity*, p. 221.

291. "*In the Comptes Rendus* (Oct. 21, 1844) *there is a report of the phenomena attending a dry trombe, or tornado, which occurred at Escalqueus.* It appeared in the form of a vast inverted cone, and was manifested in incessant rapid rotation. Suddenly it seized upon a field of maize, which it dispersed in all directions. It completely demolished a farm house, killing all the poultry. It then passed on to another farm, about two miles distant from the former, carrying off the roofs of the houses in its course. A continual dead and terrifying noise was heard by all who witnessed it, and those who were in the midst of it saw fire. In fact, *the insurance company were quite ready to admit, against their own interest, that the mischief done was the effect of lightning.*"

292. *An account of the destruction of the castle of Chatenay, near Paris, by a tornado,* June, 1839. "It has been a subject of discussion among philosophers whether those destructive meteors called tornadoes are really electrical phenomena, or whether they are caused by heat evolved from condensing vapour. The subject came under the investigation of the French Academy of Sciences in 1839, in consequence of a demand for indemnity for the devastation caused by a tornado at Chatenay, near Paris, under a contract of insurance against thunderstorms. The question was referred to Arago, the President of the Academy, and under his auspices a report was made by Peltier, *agreeably to which the insurers were called upon to pay.* The following extract from this report will convey some idea of the devasting effects of the remarkable meteor."

293. "Early in the morning a thunder-cloud arose to the south of Chatenay, and moved at about ten o'clock over the valley between the hills of Chatenay and those of Ecouen. The cloud having extended itself over the val-

ley, appeared stationary, and about to pass away to the west. Some thunder was heard, but nothing remarkable was noticed; when about midday a second thunderstorm, coming also from the south, and moving with rapidity, advanced towards the same plain of Chatenay. Having arrived at the extremity of the plain of Fontenay, opposite to the first mentioned thunder-cloud, which occupied a higher part of the atmosphere, it stopped at a little distance, leaving spectators for some moments uncertain as to the direction which it would ultimately take. Up to this time there had been thunder continually rumbling within the second thunder-cloud, when suddenly an under portion of this cloud descending, and entering into communication with the earth, the thunder ceased. A prodigious attractive force was exerted forthwith, all the dust and other light bodies which covered the surface of the earth mounted towards the apex of the cone formed by the cloud; a rumbling thunder was continually heard. Small clouds wheeled about the inverted cone, rising and descending with rapidity. *The column was terminated by a cap of fire.* To the south-east of the tornado, on the side exposed to it, the trees were shattered; while those on the other side of it preserved their sap and verdure...... Finally it advanced to the park and castle of Chatenay, overthrowing everything in its path. On entering this park, which is at the summit of a hill, it desolated one of the most agreeable residences in the neighbourhood of Paris. All the finest trees were uprooted, the youngest only, which were without the tornado, having escaped. The walls were thrown down, the roofs and chimneys of the castle and farm house carried away, and branches, tiles, and other movable bodies, were thrown to a distance of more than five hundred yards. Descending the hill towards the north, the tornado stopped over a pond, killed the fish, overthrew the trees, withering their leaves, and then proceeded slowly along an avenue of willows, the roots of which entered the water; and being during this part of its progress much diminished in size and force, it proceeded slowly over a plain, and finally, at a distance of

more than a thousand yards from Chatenay, divided into two parts, one of which disappeared in the clouds, the other in the ground."

294. The following is the explanation offered by Peltier. "In contemplating the rise and progress of this phenomenon, we see the conversion of an ordinary thunder gust into a tornado; we behold two masses of clouds exposed to each other, of which the upper one, in consequence of the repulsion of the similar electricities with which both are charged, repelling the lower towards the ground, the clouds of the latter descending and communicating with the earth by clouds of dust and by the trees. *This communication being once formed, the thunder immediately ceases, and the discharges of electricity take place by means of the clouds which have thus descended, and the trees.* These trees, traversed by the electricity, have their temperature in consequence raised to such a point that their sap is vapourised, and their fibres sundered by its efforts to escape. Flashes, and fiery balls, and sparks accompanying the tornado; a smell of sulphur remains for several days in the houses, in which the curtains are found discoloured. Everything proves that the tornado is nothing else than a conductor formed of the clouds, which serves as a passage for a continual discharge of electricity from those above; and that the difference between an ordinary thunderstorm, and one accompanied by a tornado, consists in the presence of a conductor of clouds, which seem to maintain the combat between the upper portion of the tornado and the ground beneath."—*Noad's Manual of Electricity*, p. 219.

295. In another account, in reference to the trees, it is stated, that "all those which came within the influence of the tornado presented the same aspect; their sap was vapourised, and their ligneous fibres had become as dry as if kept for forty-eight hours in a furnace heated to ninety degrees above the boiling point. Evidently there was a great mass of vapour instantaneously formed, which could only make its escape by bursting the trees in every direction; the trees were all, throughout one portion of their

trunk, cloven into laths. Many trees attest, by their condition, that they had served as conductors to continual discharges of electricity, which had vapourised all the sap they had."

296. The following extracts are from an *abridgment* of an *Account of the Hurricane or Whirlwind of the* 8*th of April,* 1838. *By Mr. J. Floyd; communicated by J. H. Patton, esq., Magistrate of the* 24 *Pergunnahs. In the India Review for July* 1838. "This remarkable whirlwind swept through the villages near the city of Calcutta (within three miles of it) on a course nearly south-east; its path being from a quarter to half a mile in width. The destructive effects as exhibited on sixteen miles of its track were examined by Mr. Floyd, in company with the magistrate, soon after its occurrence. At Codeleah many houses were destroyed, large trees torn up by the roots, and many were broken off at the stumps.—Seventeen of the inhabitants had their limbs broken, and several were killed. The villages of Sambandal and Chowbagan have been laid desolate; men, women, and children, as well as animals, have died without number, Sambandal on that day being crowded to excess by people from neighbouring villages. In other villages the visitation has been awful, but in these places it surpasses all description. As far as the eye can reach, not a house is to be seen. *The grass* (*I am at a loss to account for it*) *has been consumed. The bark of the palm trees has been peeled off as if with a knife.* These villages are inhabited chiefly by fishermen, who were, at the time, on the lake, and never felt the effects of the storm; till on their return they found the villages destroyed, and but a few survivors to account for the occurrence.

297. "The gomastah of the above village gave the following romantic account of the storm. On Sunday, at half past two P.M., the hurricane came on in a north-westerly direction, and to the best of my judgment two dark columns, that were visibly whirling round and round, descending to the earth, had the appearance of two huge daityas (or demons) preparing for combat; that a second

before they were fairly alighted they engaged in mortal strife, and agitating the waters of the lake began their work of destruction on land; that such as were in their houses hastened out to witness this wonderful phenomenon, and ere they could return to their homes, the sudden *darkness that overspread the place,* the howling of the winds, and clouds of dust attending it, rendered it impossible for them to bear testimony as to which of the two gained the victory. That from the occurrence of the whirlwind to the period it left these parts, it did not occupy twenty minutes, and *was almost immediately followed by sunshine.* There was little or no rain, *but a severe fall of hail,* which probably deprived some of life.—At Dum Dum the hailstones were uncommonly large, one weighing (as it is said) three and a half pounds."

298. "The extreme length of the storm, probably speaking, was sixteen miles; 1239 houses were destroyed; 215 men, women, or children lost their lives; 223 wounded; and 533 cattle were killed."

299. The probable agency of electricity in this whirlwind was shown by the burning of the grass, and the bark being peeled off the palm trees; this last being an ordinary effect of lightning on trees. The severe fall of hail, and the almost immediate appearance of sunshine after the darkness which overspread the place, show that condition prevailed which could hardly have existed without thunder, if the electricity had not by some means passed away from the dense clouds. It may be well to bear in mind the evidences of electric action in the dust storms of India as shown by Mr. Baddeley (109).

On the Winds and Storms of Temperate Regions.

300. I can hardly venture an opinion as to the cause of such great wind storms as have sometimes swept over England and other parts of Europe. A descent of the upper currents of air has been assigned as the cause, and calculations made as to the momentum with which such currents would descend. This view may be correct, but I cannot conceive that any violent winds can be impulsive,

especially from above downwards, as the rarer air would meet with a rapidly increasing resistance from the density of the lower atmosphere: and if storms were produced by currents of air moving with an impulsive momentum, they would cause a rise of the barometer, whereas I believe that in *all* violent storms the barometer sinks very low. This seems to show that *violent* winds are always produced by an indraught or flow of air into some space to restore a deficient equilibrium. Now what can produce such a deficiency in the atmosphere as to cause such terrific winds? I can think of no cause except that I assign as productive of waterspouts and tornadoes; and if all storms are not the result of the fall of rain, I hope to show good ground for assuming that many storms are so produced.

301. Carrying on the foregoing calculations (209) as to the rarefaction or vacuum which would result from the fall of rain; taking the same data, i.e. that water is 860 times heavier than air, the fall of one inch of rain would produce a rarefaction equal to that which would result from the abstraction of 1,997,952,000 cubic feet of air over every square mile, where, and during the time, the rain was falling. The equilibrium thus destroyed would be restored by the rushing in of the surrounding air; the force of the resulting wind depending on circumstances.

302. As the air at the earth's surface is under the greatest pressure, it would exert the greatest force in rushing into the rarefied space; and if a heavy fall of rain be from a very low cloud, the rarefied space would be so near the earth, that the rush of air might be felt at the time and place where the rain is falling; producing eddies and currents similar to what would result from the rushing of water. But if the rain be produced from clouds at a greater height, the resulting winds would prevail more or less at a distance, and no wind might be felt where the rain actually falls; there the only effect being a barometrical depression from the diminution of atmospheric pressure, owing to the rarefaction produced in the space above.

303. The fall of half or three quarters of an inch of

rain in a day is not unusual; this, over a few miles, might produce a brisk wind, or if such a rain be extensive, a high wind might result at a distance by the rush of air towards the district: but terrific winds might result from such rains as are recorded in paragraph 208. Taking the fall of rain at Genoa, the rarefaction *over every square mile* must have been equal in effect to the loss of 2,497,440,000 feet of air per hour, 41,624,000 per minute, or 693,733 feet per second; and this, on an average, for the whole twenty-four hours during which the rain was falling. Such a rain as this, if even confined to a small space, would be sufficient to produce a violent local storm; but if very extensive, would seem sufficient to produce the most violent storm on record. Even this fall of rain was exceeded in some of the other cases given.

304. The fact that such a large quantity of rain falls in one district in a day tells in favour of the theory, as it shows there must be a current of air and vapour toward the district, otherwise so large a quantity of water could not accumulate over any one spot within so short a space of time.

305. I have been led by a long course of observation to these opinions, and not by theory alone; and that a vacuum is produced by the formation of rain is shown by many trifling facts which are of frequent occurrence. I have often seen, and pointed out the fact to others, that when a heavy but compact shower has been passing at a short distance, the smoke from chimneys has been carried towards the cloud in a very remarkable manner, and changed its direction as the cloud moved on. It is quite a common opinion amongst country people that thunderstorms always come against the wind; and I believe it is generally the case that the surface-wind blows towards a thunderstorm from all points, if there be no brisk and general wind at the time. I have several times noticed that during seasons when excessive rains have prevailed for a time in France and other parts of the continent, the wind in England has generally blown towards those parts, often with considerable violence; and sometimes this

phenomenon has been strikingly confirmatory of the theory.

306. The whirling or undulating of the edges of clouds, as noticed during the hailstorms at Clermont (257 and 263), is strikingly indicative of inward and upward currents of air: it may be often observed on the approach or passing off of a thunderstorm; and the following extracts show that the same phenomenon is very remarkably exhibited during the first ushering in of the Indian monsoons. "As the storm gains head, it is noticed that the sheet of falling water which forms its van is very regular in its outline, forming apparently a straight line over a wide space from north to south. Over head the sky, as it has been all the morning, is greyish blue; the black mass of cloud, elevated between 2000 and 5000 feet, advancing steadily with a determined front, small patches of black vapour, detached, or rapidly forming on its outer edges, are seen violently agitated, hurrying to and fro as if uncertain, *and then drawn in vigorously beneath the advancing edge of the general mass*: we can no longer distract our attention from the gloomy expanse of the descending torrent which presses onward; it is seen to fall rather behind the vertical at first *as if sucked in a little,* then to curve boldly outward in advance, pressing from beneath a hot blast of oppressed air; and finally, as the friction against the surface of the earth retards the movement of this latter, to be affected only by its gravity in the last moments of its descent, so as to fall with but trifling obliquity."—*From a MS. on the Climate of Ceylon, by Dr. R. Templeton.*

307. Many cases might be adduced of wind blowing from different points towards the place where heavy rain or hail was falling. The following account of a storm in North Staffordshire is taken from the Report of the British Association for 1856. This storm, which came from the north-west in the afternoon, between four and five o'clock on July 22nd, 1855, "continued with great violence for about half an hour. Some of the masses of ice which fell being an inch and a half in diameter. The

storm was attended with gusts of wind and thunder, and was of a very *limited and defined extent;* but to the south of the writer's residence, about four miles away, near the Barlston Station, a violent wind from an opposite direction, south-west or south-south-west, occurred at the same hour, without rain or hail, the ravages of which could be traced for a length of two miles, with a breadth of only from fifty to one hundred yards. Oaks were deprived of their largest limbs; poplars broken at the height of eight or twelve feet from the ground; and an alder, fifty feet high, was uprooted and carried some distance. The clouds were extremely dark for a great extent of country."

308. A storm occurred on the 22nd of September, 1856, which affords important data for investigating the cause of storms, as it passed nearly over the Radcliffe Observatory at Oxford, where the meteorological phenomena are continuously registered by photography; and as the storm continued only a few minutes, and during that time the barometer, raingauge, and anemometer were simultaneously and violently affected, their indications could not have been correctly registered by any other means. From information afforded me by several gentlemen, I was enabled to trace the storm throughout its course, and collect many particulars respecting it, which I submitted to the Ashmolean Society November 3, 1856. Letters respecting this storm appeared in the "Times" September the 26th and 30th, and October the 13th.

309. The storm commenced a mile or two from Glastonbury, being accompanied by dark clouds and heavy rains; it passed over Glastonbury in the direction of the wind, from the WSW; the wind was high and the rain heavy to some distance on each side of the storm, the destructive effects of which did not extend over more than about 100 yards in width, but there its effects were tremendous, unroofing buildings, uprooting trees, &c. Its violence lasted only a few seconds. From Glastonbury its course was by North Wootton, Warminster, Croscombe, and Radstock; thence to Clyffe Pypard, Shrivenham, Buckland, Cumner, and Elsfield, passing between Oxford and Summer Town

(the storm being more or less felt at all these places). This course, although generally in the same direction, is far from a straight line, and still further from a regular curve. The storm passed Glastonbury about two o'clock P.M.; from thence to Oxford, where it occurred at twenty minutes past four, the distance is about seventy-six miles.

310. During the passing of the storm at Glastonbury, "a person was sitting in a room, which, besides an outward window, had an inner one, looking into another room or passage; when the storm came on he felt for a moment or two as if he could not breathe, and at the same time a pane of the inner window was blown in and across the room. So it would seem that the air in the room had been drawn up through the chimney. The outer window was not injured." This fact is important, and affords an explanation of phenomena of the storm which would otherwise have been inexplicable.

311. There were many cases throughout its track which give evidence of a current acting upwards with great force; and in others a like evidence of a downward crushing effect. These cases may be readily accounted for as the effects of a high degree of rarefaction at times within the passing clouds, which causing a like rarefaction of the air beneath them, when the consequent rush of denser air into the rarefied space would produce such effects as were caused by the storm. In one instance, when passing a hill near Glastonbury, "on the side which faced the storm it evidently rose, taking off the *tops* of two elms, and all but clearing the rest of the hill. On the other side it descended again, and swept down towards the lowlands between Glastonbury and the Mendips; here the grass looked as if a strong stream of clear water had passed over it." This case is strictly in accordance with the cause assigned. Two men in a stableyard in Glastonbury were driven by the wind against opposite sides of the yard at the same moment; and supposing the air in the yard to have been highly rarefied by the passing of the

clouds, the rush of denser air into the space would account for the men being driven in opposite or any directions.

312. At Clyffe Pypard the storm was very violent, but chiefly confined to a tract of about a mile in length, and three or four hundred yards in width. At half past three it came on from the SSW with "very heavy rain." "This place is situate immediately under a steep hill, which is to the south of it, and has usually been remarkably free from storms. The storm did not last more than three or four minutes; its effects first appeared a few hundred yards above the hill, where several large trees were blown down or broken off; it then swept down a fir plantation on the side of the hill, snapping off or tearing up seventy trees of fifty years' growth, and breaking *and crushing in a singular manner* (as if from a heavy fall of snow) the heads of many large beech, oak, and ash in the copse adjoining. The injuries to the trees above, and on the side of the hill, and in the *first* field below it, were in the *direction of the storm;* but in the next field, where twenty large trees were uprooted, and a great number broken, the wind seemed to have whirled them in all directions." These effects seem similar to those on the hill near Glastonbury; the trees on the top and side of the hill being blown down or broken by the direct current of the storm, while those at the bottom of it were crushed and dashed about by the heavier air rushing into the valley, where the air had become rarefied by the passing clouds.

313. The storm at Buckland is thus described: "It lasted some seven minutes, its greatest fury between two and three minutes only; before the storm came on it had been raining heavily, and gradually grew darker and darker, when suddenly the rain came down in one sheet of water, so thick and so fast, that objects could not be seen at fifty yards' distance. With other mischief, the wind tore the lead from the buildings, whirling the slates into the air, and carrying them from twenty to two hundred yards into the fields."

314. When passing Cumner and descending the hill, the

damage from the storm was not great, but at the bottom of the hill, about where the Cumner road joins the road to Eynsham, the storm was again very violent, and the rain descended in torrents; at the cottages near this spot much damage was done, and many trees were blown down within a short distance of them. At Botley also the wind did some mischief. From thence the course of the storm was over Medley and the lower part of Port Meadow, passing just to the NNW of Oxford.

315. The Radcliffe Observatory is rather out of the line of the storm, which was also comparatively light in the neighbourhood, yet the effects on the meteorological instruments were remarkable. The barometer had been sinking gradually the whole day previous, the photograph showing a regular and tolerable straight line till the passing of the storm (which did not continue more than five or six minutes), when it suddenly sunk, showing a diminution in pressure of seven hundredths of an inch, and then it more suddenly rose again to its former level. The anemometer shows that for some hours previous to the storm the wind was blowing briskly, but for about half an hour before it commenced the wind rather lulled. The first gush of the storm blew out the gaslight connected with this instrument, which fortunately was discovered within five or six minutes; there is therefore a blank in the photograph for the interval; but it is evident that the wind during that time increased in a very violent degree. After the storm had passed, the wind sunk to about the same rate as previous to the half hour before the storm. *There was no change in the direction of the wind during the storm; and the thermometer was not affected by it.* The photograph of the raingauge being taken by the same light as that of the anemometer, there is a like blank for the five or six minutes, but it shows that during that time the fall of rain was about the 10th of an inch.

316. After passing Oxford the storm again increased in violence; it did not blow in one sweeping current, but in gusts, doing mischief here and there; while trees, &c. close

adjoining, and in the direction of the wind, escaped uninjured.

317. The storm then passed on to Elsfield, which being situate on a hill, and *facing* the storm, was fully exposed to its fury. Here within a small space six or seven large trees were blown down or broken off; the top of a large elm was blown off, and carried over a wall some ten yards or so, into a garden, and there stuck upright as if planted. These effects give evidence of a strong upward current, as the higher trees only were injured, the shrubs and lower plants escaping. The rain during the time was tremendous. While passing the top of the hills the storm again swept the ground, as the leaves were stripped from the turnips in the fields. The storm seemed to have ceased here, as I could learn nothing more of its effects, except that heavy rains occurred in the direction it passed off in.

318. The phenomena of the storm show, that throughout its course it came on in the general direction of the wind; it was accompanied throughout by dark heavy clouds and enormous rains; *there was no increase of the wind till the clouds came over; the violence of the wind and excessive fall of rain were in all cases simultaneous;* and as soon as the *heavy rain ceased,* the wind again subsided to about the rate it had blown at previous to the storm. The storm appears to have been direct and not whirling; near Glastonbury, where it was described as a whirlwind, out of many trees examined, "most of them were thrown down in the direction of the storm—some sideways, both right and left, and only one backwards;" the like seems to have been the case throughout its whole course; and *it is certain there was no whirlwind as the storm passed Oxford.* It seems therefore that to whatever cause the general wind may be ascribed, the storm could not have arisen from any great atmospheric disturbance, resulting in a whirlwind of considerable extent, to which cause storms are now generally assigned. It could not have been from change of temperature, as there was little or none.

319. The distance traversed by the storm in a little

more than two hours was about seventy-six miles. This is the speed of a "swift wind" only, and not a "hurricane." Something beyond this must have happened to produce the mechanical effects recorded; and as it is evident that the storm was connected with and dependent on the passing of the heavy clouds and fall of rain, I cannot see how any cause can be assigned for it, except the rarefaction produced in the cloud by the fall of rain. Bringing the theory (already advanced) to bear on the phenomena of this storm, it is certain that the fall of rain at the Radcliffe Observatory was the tenth of an inch in six minutes, thus causing a rarefaction over every square mile where such rain was falling, equal to the abstraction of more than thirty-three millions of cubic feet of air per minute; and as the rain was probably much greater in some places where the storm occurred, the rarefaction must there have been greater, and apparently sufficient to account for the effects of the storm.

320. It is probable that all heavy rains produce a like rarefaction; but the small elevation of the clouds in this storm will account for the rush of air being felt on the earth to such a degree. On enquiring of a labouring man as to the appearance of the clouds as the storm passed, I received the following expressive answer:—"They (the clouds) seemed right bang down, as if you couldn't get under them;" and respecting the rain he said, "it did'nt rain at all, it came down any how." I find no notice of any thunder during this storm, or any other of the heavy storms which occurred about the same time, except in that at Dublin (324), and even there it does not seem to have been very heavy. This fact may be advanced as a further proof that during excessive rains the electricity passes from the clouds in a wholesale manner, and not merely by disruptive discharges.

321. Through the kindness of the late Radcliffe Observer[p], I am enabled to exhibit the barometric and ther-

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[p] As the very sudden and lamented death of Mr. Johnson has occurred since this work has been in the press, I cannot refrain

mometric curves as they occurred at Oxford on the day of the storm. The lines *Bar.*, *D.B.T.*, and *W.B.T.*, in fig. 1, show respectively the curves produced by the barometer, the dry bulb thermometer, and the wet bulb thermometer, from two P.M. September 22, to two P.M. September 23. The larger notch in the line *Bar.* shows the sudden fall of the barometer at the time of the storm; the sinking, although very rapid as the storm came on, was still gradual; but the rise was far more sudden, from the rush of air into the rarefied space as the storm passed on. The "thermographs" not only show that there was no change of temperature worthy of notice at the time of the storm, but also that the temperature throughout the day was very even. In fig. 2 the line *B.* 2 represents the barometrical changes on the day previous to the storm, the fall being from 29,71 at 2 P.M. on the 21st, to 29,14 at 2 P.M. on the 22nd. The line *B.* 3 shows the level of the barometer on the day after the storm. The line *Ther.* exhibits the thermometric curve produced from 2 P.M. on the 28th of June, 1855, till 2 P.M. on the following day; when the weather was fine, and the fluctuation far from extreme: I give it as a contrast to the *very* even temperature exhibited on the day of the storm[q].

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from offering a tribute of respect to that gentleman's memory for many favours conferred on myself. Every facility for consulting the meteorological registrations at the Observatory, or the books in the library, has been afforded me at all times; and discarding all class distinction, he has often discussed with me many points connected with the subject of this essay, for the publication of which he was very anxious. Under his auspices meteorology has become an important subject for investigation at the Observatory, and the photographic process now in operation has been rapidly approaching to perfection. The value of the process cannot be too highly appreciated, as being continuous no phenomenon can escape observation; of this the registration of the phenomena of the storm under discussion affords ample proof.

[q] As the photographic process for registering meteorological phenomena may not be generally known, I give the following brief description of the daily process in taking the barograph. A

322. About the time of the storm of the 22nd of September there seems to have been a general break up of the previously prevailing fine weather, and tremendous storms, accompanied by very heavy rains, occurred in various parts of England, Scotland, and Ireland. In many cases there were *simultaneous* storms in different places. During my enquiries respecting that storm I was informed of several violent storms, with tremendous rains, in various parts of the country; I made no note of them, as they were not connected with the subject of my enquiry; but the columns of the "Times" of the last week in September, and the first three weeks in October, 1856, give ample proofs of the violent storms and hurricanes at the time.

323. On the night of the 26th of September and the two following days very violent storms occurred at different places, and all of the same character; that is, *blowing on-shore, and to where tremendous rains were falling*. The following extracts refer to some of them:—

324. From the "Dublin Freeman," Monday, Sept. 29: "We do not remember an autumn season during the last

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piece of photographic paper is passed by clock-work in the 24 hours, in a horizontal direction, behind a screen in which there is a perpendicular slit; in front of this slit a gaslight is kept constantly burning, and between the slit and the paper the tube of the barometer is placed: then as the paper is passed along, the mercury rising or falling in the tube protects more or less of the paper from the action of the light, which renders the whole dark which is exposed to its influence, and thus an exact picture is taken of every fluctuation, and the moment of its occurrence indicated by the dark edge marking the height of the mercury as the paper passes behind it. By a similar process registrations are taken of the wet and the dry bulb thermometers, the fall of rain, the velocity of the wind, and its direction; and lately an addition has been made to the apparatus at the Radcliffe Observatory, by which the electric condition of the atmosphere is photographed. The photographic instruments, and the manner of using them, are described in the fifteenth and sixteenth volumes of the Radcliffe Observations.

twenty years wherein mildness of temperature, and even heat like that of summer, have been so suddenly succeeded by cold, wet, and tempestuous weather, as during the present month. The storm so long brewing seemed to burst forth in all its violence on Friday night (Sept. 26), when a tempest of wind and rain—unequalled for many years in our memory—visited, as we understand, the eastern coast of Ireland. The gale on Friday night did not reach its full intensity for some hours, at least at Dublin. At or about the hour of one o'clock on Saturday morning the storm reached its fullest fury and intensity. The wind could scarcely be said to come from any distinct quarter, but rushed in fearful gusts, howling and raging through the empty streets, as it seemed, from every point of the compass. *The rain came down in unbroken sheets of water*, and to add to the horror of the night, several flashes of lightning broke forth, giving a momentary and dismal illumination to the scene. Fortunately for the safety of property in Dublin and its vicinity, the storm did not continue at this pitch of fury for any length of time; but it was in the direction of the coast that the most serious fears were entertained for the fate of every vessel afloat in the channel at the time. We understand that outside the force of the gale was felt from the *southward* and *eastward,* veering occasionally somewhat to *the west*. Dublin river presented yesterday several fearful illustrations of the fury of the storm, the surface of the water being covered with masses of agricultural produce, hay, corn, and even dead animals; also large trees which had been torn up by the roots." A long account follows of the violence of the storm on the coast.—*Times, Sept.* 30.

325. "Dublin, Tuesday, Sept. 30. The papers are filled with details of the casualties consequent upon the terrific gales of Friday and Saturday. The storm seems to have been confined to the east and north-east coasts. *At Belfast it was severely felt*[r]. A letter from Drogheda says:—The

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[r] The following is an abstract from a meteorological register

Mattock river swelled far and away beyond anything known before, and eventually the accumulation of water bore down upon the bridge which spans the Mattock at its discharge into the Boyne, and broke down that structure."—*Times, Oct.* 1.

326. At the *very time* the storm was blowing at Dublin and on the eastern coast of Ireland from the *southward* and *eastward*, a gale, with heavy rain, was blowing in Devonshire from the NW. The following is from an account with which I was favoured by M. A. Mathews, esq., Merton college:—"Friday the 26th had been a tolerably fine day, with no wind blowing whatever; but towards evening, and especially shortly after sunset, a black and heavy bank of clouds rose slowly from the west and north-west, and gradually spread over the whole sky. About seven o'clock there was a slight breeze and a fine rain. However as the night grew the rain and wind increased, and towards eleven P.M. the latter blew so strong as to be quite a hurricane, while the rain lashed down in a continuous stream. The night was the darkest I ever remember to have been out in; the darkness was so thick as to be as it were palpable. The wind did not blow steadily, but came in violent and sudden puffs every minute or so. I think it owing to this fact that so many trees were blown down in our neighbourhood; for a tree would bend and yield before a constant strain, but would be surprised, as it were, by any sudden application of force to it, and fall unprepared. The violence of the gale abated somewhat towards morning, and at daybreak there was no rain; the

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at Belfast, and shows a remarkable coincidence between the force of wind and the fall of rain:—

Date.	Bar. 9 A.M.	Dir. of wind.	Force of wind.	Fall of rain.
Sept. 25	29·280	N	0·1	0·090
26	29·360	SE	0·0	0·0
27	29·113	E	0·4	1·020
28	29·225	NE	0·3	0·820
29	29·339	N	0·1	0·220
30	29·620	N	0·0	0·0

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wind, however, was still pretty strong, and *now* kept blowing continuously from the north-west."

327. "A destructive storm passed over Taunton soon after midnight on Friday (Sept. 26th), and was attended with considerable mischief from the violence of the wind. The capitals of the roof of the Crown Court at the new Shirehall, which have been fixed some time to the stone corbels, were lifted *en masse,* and many of the massive timbers completely turned round and crushed to pieces, the *débris* being thrown about in all directions. The interior of the court presented a sad picture, the fine ponderous principals and bearers of immense proportions having been thrown down, and from £100 to £150 worth of timber and labour destroyed. It is impossible to conjecture in which direction the wind took the roof, as the broken pieces were carried into the most unlikely places; the gable rafter was blown outward on the north side of the building, where it still hangs in dangerous suspense. Strange to say, neither the roof of the Nisi Prius Court, nor the light skeleton timbers of the turret, were in the least disturbed by the tornado, which seems to have been very partial in its effects. *The low lands in this neighbourhood have been under water, through the heavy rains of Friday night and Saturday.*"—From the "Taunton Courier," Oct. 1, 1856. This storm may have been a continuation of the one from Barnstaple, being on the same night.

328. The following are short extracts from the "Times," all telling the same tale; i. e. of torrents of rain and violent hurricanes, all blowing more or less from the sea on to the land, and affording fair grounds for believing that the winds were the result of the heavy rains. There also appears to be no connection between the storms, but that each blew towards the point where the rains were falling.

329. "The weather in the south-east of Scotland, Edinburgh, Sept. 29. From the 20th or the 21st the weather may be said to have broken up, and the rain for some days in the beginning of last week continued to fall incessantly, and sometimes in torrents. The 26th witnessed a

favourable change, but it was only for a day. On Saturday the 27th the wind returned to the eastward, and towards the evening of that day rose to a perfect gale. All night through the gale continued, and instead of abating yesterday (Sunday) forenoon, it seemed only to have increased in fury. The water of the Leith—a stream rising in the Pentlands, and debouching at Leith harbour—has risen to a height that is said to be unprecedented for the last twenty years; it has brought down numerous carcases of sheep as well as trees, bridges, &c."—*Times*, *Oct*. 1*st*.

330. " On Wednesday, last week (Sept. 24th), the storm, which was noticed in the ' Times' as having begun to subside in the north of Scotland, gradually increased again until the wind from the north-east rose to a hurricane, and the rain fell in torrents. On Friday, Saturday, and Sunday (26th, 27th, and 28th), the storm continued without abatement; and in consequence the rivers were all flooded, and great damage has been done." The " Aberdeen Journal" says, " We do not remember in the north such a heavy fall of rain, and so great a destruction of the crops, since the memorable '29. The Dee brought down a large quantity of sheaves on Monday. Donside has especially suffered. The river rose at the Kintore station nearly up to the embankments." The " Banffshire Journal" says, " The Spate has brought down considerable quantities of timber," &c. Similar statements are given respecting the Spey, the Lossie, and the Isla. " The river is so much swollen at the villages of Kingston and Garmouth, that communication is maintained between them by means of boats. A farmer on the banks of the Spey lost a number of cattle, besides crops, &c. Between thirty and forty yards of the Inverness and Nairn railway were swept away."—*Times, Oct*. 3.

331. " Worcester, Sept. 27. A terrible equinoctial gale, with floods of rain, took place this morning."—*Times, Sept*. 29.

332. " Shields, Sunday night (Sept. 28). A very heavy gale from the eastward has prevailed on the north-east coast during the last forty-eight hours; blowing with

great violence last night, and a very fearful sea is rolling on the coast. The Tyne is much flooded, the heavy rains having sent down the 'freshets' from the west."—*Times, Sept.* 30. A long list of disasters at sea follow these extracts.

333. "Shields, Wednesday, Oct. 1. After four days of rain and wind, such as are rarely experienced in these somewhat stormy latitudes, we have to-day a delightful change in the weather." The account gives a long list of disasters from the storm, and amongst others it is stated that "the rain washed down about 40,000 tons of ballast upon the Pontop branch of the North-Eastern railway." —*Times, Oct.* 3.

334. "Dover, Sept. 28. The equinoctial gales have set in during the past week with unusual severity. Day by day they have increased, until last night it blew a perfect gale of wind from the SSE. During the night the gale became stronger, and the sea got up to a frightful height, but this morning the aspect of the weather was most appalling." Near half a column is given in the "Times," Sept. 30, respecting this storm at Dover and Brighton.

335. At Uckfield Observatory upwards of two and a quarter inches of rain fell from nine A.M. on the 27th to nine A.M. on the 28th of September, being more than had been known to fall there in twenty-four hours for at least fourteen years past.

336. Many other extracts might be given, all bearing on the subject, as long accounts were given in the newspapers at the time; but I believe quite enough has been advanced to show at least the probability that these violent storms were the result of the enormous falls of rain at the time.

337. With respect to the more general winds of *temperate* regions; changes of temperature, or the shifting of the upper current of air, which undoubtedly prevail from the equatorial to the polar regions, may be assigned as causes; the rising of vapour must also produce some effects in displacing the air; but I believe that, except in mountainous districts, the principal cause of all winds in-

temperate climates is the fall of rain and the resulting rarefaction of the air.

338. The fact that the barometer sinks during high winds, shows that the rush of air is produced by some vacuity or deficiency which it tends to fill up, and that the causes producing such winds must be sought for in the quarter to which they blow.

339. The equinoctial gales have the same general direction from west to east in spring, when the change of temperature in the quarter to which they blow is from cold to warmer, as they have in autumn, when the change of temperature is just the reverse; and it seems improbable that such opposite causes should produce the same effects. No such objection holds against the opinion that these gales are the effects of extensive rains in central Europe; in spring after the breaking up of the long and dry frosts of winter, and in autumn after the hot and generally parching summer has passed. But these questions can never be satisfactorily decided except from careful and extensive observations on the continent of Europe.

On the Hurricanes and Storms of Tropical Regions.

340. On entering on this question I feel that I am venturing upon dangerous ground, as I know but little of tropical phenomena, my attention having been more especially directed to the climatology of the polar regions in reference to the cause of terrestrial magnetism and the aurora. Still, although under such disadvantages, I consider it necessary to take this subject with the rest, and endeavour to show that the proposed theory will afford a fair explanation of both the cause and the course of tropical storms. All my views on this subject are the result of mere theory, and as such I submit them. I have no observations of my own to fall back on for support, and I have had no time to devote to lengthened investigations of what has been observed by others; still I hope the opinions I advance may be worthy of consideration, although opposed to generally received theories and acknowledged authorities. Even if I am in error I may do good, if it

is only in putting the subject in a new point of view, and thus enabling others to throw more light upon it. However, the importance of the subject will excuse my temerity.

341. As to the hurricanes of tropical regions, I know of no reason for attributing them to any other cause than those assigned for the thunderstorms of temperate climates; the only apparent differences being in the greater magnitude of both cause and effect in the former; and as the most violent thunderstorms in Europe occur in the hottest seasons, it is reasonable to expect that such storms should prevail with greater frequency and more terrific effects in tropical regions than in more temperate ones. But before I go further into the question, it will be necessary, for comparison, to give a few extracts descriptive of the phenomenon.

342. "The following description will convey to the mind of the reader some idea of the violence of a West Indian hurricane. It is taken from various accounts of the great hurricanes in Jamaica of 1780 and 1784, communicated by respectable eye-witnesses. A hurricane is usually preceded by awful and certain prognostics. An *unusual calm prevails; not a breath of wind is felt;* the atmosphere is close and sultry, the clouds wild, broken, and perpetually and rapidly shifting; at length a deep and portentous gloom gradually settles and overspreads the hemisphere; the sun is enveloped in darkness; a deep, hollow, and murmuring sound is indistinctly heard, like the roaring of a distant cataract, or the howling of winds through distant woods; rapid and transient gusts of wind and rain speedily succeed; various birds are seen hastily driven along the sky, or are thrown down by the violence of these gusts; even the cattle grazing in the fields, as if instinctively aware of the approaching danger, withdraw to the adjacent thickets for shelter. The blasts soon become more impetuous; at one moment they rage with inconceivable fury, and the ensuing instant seem as it were suddenly to expire. In a few hours the hurricane reaches its acme of violence, when all the winds of heaven, and

from every point of the compass, winged with destruction, seem let loose from their caverns."—*From Stewart's Past and Present State of Jamaica.*

343. An account of the hurricane in Barbadoes on Thursday August 11, 1831. "The appearance of Wednesday evening (August 10) was indicative of unsettled weather, and many persons prognosticated that there would be a gale before the morning; but few anticipated a visitation so dreadful. The wind was at times rather high, and about ten o'clock there was a shower of rain, which was succeeded by a calm. After this a *dense mass of clouds gathered over the horizon*, and remained for some time suspended in gloom. At twelve they burst in a severe squall, which was followed by *a heavy rain.* At this period there was a smart breeze from north-east, and the wind began to increase. In two hours it blew a tremendous gale, but moderated for a short time, when it suddenly became more violent, and was a perfect hurricane at three o'clock. Now the work of destruction commenced. From this hour until five it raged with unparalleled violence, whilst the lightning would at intervals cast a momentary but horrid glare on the mangled objects around. The houses were either levelled with the earth or unroofed; the largest trees torn from their roots, or broken as reeds. Numerous individuals were buried under the ruins, or exposed without shelter to the pelting of the storm, and threatened with instant death at each successive blast that hurled the shattered fragments in every direction. The majestic cocoa-nut trees would be tossed to and fro as a withy, then snapped off with an appalling crash, or driven from the earth with terrific force. The wind had now veered to east, back to north, and to north-west; again it shifted, and blew fiercely from east, veered to south-east, and about six o'clock burst from south-west with renewed violence, accompanied by torrents of rain. Once more the hurricane raged, but there was little standing to encounter its fury."—From the "Barbadoes Globe" of August 15, 1831, as quoted in *Bayley's West Indies*, p. 710.

344. I might perhaps have selected better and more elaborate accounts of hurricanes, but the foregoing are the most ready at hand, and seem simply descriptive of phenomena without reference to theory respecting them. The first is a description of the actual commencement of hurricanes; the latter describes the coming on and effects of a hurricane already in action, or partly so. It was first printed on the spot where the most destructive effects were produced, and only four days after the dreadful visitation.

345. Hurricanes have been generally attributed to a sudden condensation of the heated atmosphere; but it is difficult to conceive the possibility of any natural change of temperature producing such sudden and violent winds, or that such tremendous effects could result from changes of temperature alone. I believe that in some hurricanes a great degree of cold has been experienced, but hurricanes do occur without any such effects; therefore the *occasional* occurrence of cold at such times may be considered as an accidental effect of the phenomenon, rather than the cause of it. The awful and gloomy prognostics which *always precede* the *beginning* of a hurricane, seem to tell against the idea that change of temperature is the sole or even a principal cause of it; and the length of time a hurricane will continue seems to show that some other element is a powerful agent in producing the phenomenon. The theory that hurricanes are enormous whirlwinds I pass for the present, that theory being more connected with the course of storms than with the cause of them: I will merely observe that the changes in the direction of the wind in the Barbadoes hurricane were not regular as indicative of a whirlwind, and the changes were not greater than what occurred during the storm in London (219). But there may be no analogy in phenomena so different in magnitude.

346. Look on the subject how I will, I can see no difference, except in magnitude, between the hurricane and the thunderstorm. If we compare the foregoing description (342) of the commencement of a hurricane with Beç-

caria's description (205) of the commencement of a thunderstorm; there are the like gloomy prognostications, the unusual calm, the close and sultry atmosphere, and the wild ragged clouds. Before the hurricane came on at Bridgetown "*a dense mass of clouds gathered over the horizon,*" and at midnight they burst in a severe squall, followed by a *heavy rain.*

347. As the heavy clouds pass onward the hurricane attends them; and this hurricane, after leaving Barbadoes, passed over the island of St. Vincent (distant about eighty miles). "But before it reached St. Vincent, an inhabitant of the island, named Simons, had ridden out at daylight, and was about a mile from his home, when he observed *a cloud to the* north of him, of an olive green colour, *more alarming in its aspect than anything he had ever seen during his residence of forty years in the tropics.* Mr. Simons therefore hastened home, and by nailing up his doors and windows saved his home from the great calamity."—*The Tempest*, p. 170.

348. "The advance of the wind in the northern quarter of the island on Thursday is described as most terrific. In vain did man by art construct edifices which he relied upon as capable of resisting the fury of the elements. The wind passed over them, and they were no more to be seen. The stoutest and the hardiest trees were uprooted like saplings, and scattered in the air like twigs. Houses were lifted up entire, and thrown to an incredible distance from their foundations. The memorable hurricane of 1780, although its approaches were more awful, attended as it was by loud peals of thunder, incessant rain, a roaring sea, and vivid lightning, *none of which accompanied the late storm, save heavy rains,* was far less destructive."—*From the St. Vincent Gazette of August* 18, 1831, *as quoted in Bayley's West Indies*, p. 724.

349. "But the most remarkable phenomenon which took place at St. Vincent was the effects of the storm on the extensive forest which covers a great part of that island. A large portion of the trees at its northern extremity were killed without being blown down. They appear to have

been killed, not by the wind, *but by the large quantity of electric matter rendered active during the storm.*"—The Tempest, p. 171.

350. I believe the foregoing extracts fully support the opinion that all the phenomena of hurricanes are those of thunderstorms magnified to a terrible degree. They are always attended by heavy clouds and rain, and lightning more or less. The effect on the trees at St. Vincent's was similar to that on the trees at Chatenay (292). I have already shown that thunderstorms are productive of violent winds; and if such is the case in the temperate climates of Europe, considering the enormous accumulation of vapour and torrents of rain in the tropics, the tremendous winds in hurricanes may be fairly attributed to the same cause; that is, the rarefaction of the air from the precipitation of rain.

On the Course of Storms.

351. I have had so few opportunities for acquiring a knowledge of what has been advanced on this subject, that I ought perhaps to pass it altogether; and in fact it was my intention to do so when I commenced this Essay. I therefore beg that the opinions I advance may be looked upon as mere suggestions, submitted for the consideration of those who are qualified to take up the subject, rather than as opinions advanced on a well investigated question. My object being to put my theory forward on every phenomenon connected with rain, and evade no point bearing upon that subject.

352. As the theory I propose is directly opposed to the cyclone theory of storms, it is necessary that I should show in what manner I believe that theory fails, before I endeavour to show the correctness of the one I advance. *In the first place, I know of no power operating in nature which can, in my opinion, produce a whirlwind of several hundred miles in diameter, with a continuance of several days, over a course of two or three thousand miles. For if the air within the supposed circle were deficient in density, it seems to me opposed to all natural laws, that a whirlwind of the*

kind should commence, and go on spreading out to such an extent, and this for several days, without the air exterior to the circle rushing in to restore the equilibrium. If the whirlwinds are impulsive, i.e. that the body of air forming the whirling portion is cutting its way through the atmosphere from an impulsive force, then I conceive that it would rise upwards, and spend itself where there is less resistance, and not force its way through the very densest part of the atmosphere, at the surface of the sea. Cyclone storms have been explained as similar to vortices in water, but vortices avoid the place of most resistance, and rise to the top where it is the least. Currents in the ocean seldom penetrate to any great depth, and certainly decrease in velocity from the surface downwards.

353. The fact that the velocity of the wind in a hurricane is vastly greater than that of the progress of the storm in its onward course from place to place, is no proof that such storms are whirlwinds, as the Glastonbury storm (308) proceeded from place to place at the rate of thirty-four miles in the hour; whereas the velocity of the wind as the storm passed was that of a hurricane, and that storm had nothing of the character of a whirlwind in the neighbourhood of Oxford.

354. The effects of West Indian hurricanes do not appear to extend to any great height in the air, as the hurricane clouds (as described to me) show no sign of disturbance when seen at a distance from a tolerable elevation, but appear like dense, solid masses; terrible to the beholder from a foreknowledge of the coming effects, and offering to the eye no indication of the wild commotion beneath them, or the destruction which attends their path. How is this to be explained if hurricanes are whirlwinds? surely the whole mass of clouds should go round in the vortex if there were one. But look at the phenomenon how we will, all seems to point to the clouds as productive of the storm, and to show that violent winds result from causes produced by the clouds, these causes being renewed and sustained as the clouds move on.

355. I believe the advocates of the theory offer no explanation as to the causes producing cyclone storms, but ground their opinions on observations on the course they take; *but I further submit that the evidence adduced on the subject is insufficient to prove the rotation of hurricanes, or the distance over which they are said to travel in their entirety, and the enormous diameters ascribed to them.* I have no doubt of the correctness of the evidence offered, but storms and atmospheric changes so often occur at or about the same time in places far apart, that I am led to believe the conclusions so often arrived at, that vessels many miles apart, and encountering storms about the same time, were in reality in one and the same storm, are not to be admitted as proved, and are often erroneous.

356. On the day of the tornado at New Brunswick (282), two others of a similar character occurred in the neighbourhood, one seventeen miles off, and the other at about the like distance beyond it.

357. If these tornadoes had been over the sea, and struck different ships, it is probable that they would have been considered as one and the same storm, and therefore very false conclusions arrived at respecting their duration; or a slight deviation in the direction of the course of the one from the other might have been advanced as a proof of their cyclone character, as no one would have ventured the opinion that two or three storms of such an extraordinary character could have occurred in one day.

358. I have shown (323) that on the 27th of September, 1856, storms occurred at the same time at various places on the coast of England, Scotland, and Ireland; but all evidently disconnected, and produced by local causes. Had different vessels fallen in with these different storms, they might have been taken for one and the same storm, and been advanced in proof of the cyclone or any other theory. The tendency of peculiar conditions of the atmosphere to produce similar and striking phenomena simultaneously in various places is often remarkable. After a long con-

tinuance of hot weather many thunderstorms sometimes occur at the same time at places far apart.

359. Dr. Buist, in his account of *Remarkable Hailstorms in India*, in the *Report of the British Association for* 1855, says, "But the frequency with which hailstorms occur simultaneously at places remote from each other, but nearly in straight lines, seems to indicate a tendency on the part of the column to become continuous. The most remarkable of these are the hailstorms which fell on the 12th and 13th of May, 1853, at Ferozepore, Lahore, and Meean Meer, Peshawur, and Jummoo, places occupying a line of 350 miles in length, right across the Punjaub; unluckily, the hours at which they occurred at these places respectively are not given. Although this is the only instance I am aware of, of a series of hailstorms bursting out simultaneously, and if not quite forming a continuous line, appearing somewhat like a string of beads stretched across the country, we have a number of them occurring in pairs, or in threes, on the same day, at places remote from each other. Our first outbursts of hail nearly always happen within a week or two of each other; and I have no doubt that in many of these cases it would appear that there had been independent chains of hail-showers, or of local atmospheric changes, many of them accompanied by hail, had a greater abundance of records for reference existed. The following examples of this will be found in the printed list:—

Lohargong ...... Bhopalpore	60 miles apart, Feb. 9, 1825.	
Jaulna .... Aurungabad ...	75 miles apart	May 14, 1849.
Deesa	350 miles from latter	
Kurnaul .... Simla	100 miles apart	May 3, 1849.
Peshawur	400 miles from Simla	
Ootacamund ... Nursingpore ...	50 miles apart, May 20, 1852.	
Hydrabad (Sind) Delhi	500 miles apart, April 17, 1854.	

360. In the year 1848, in a paper read at a meeting of the Ashmolean Society, I endeavoured to show the necessity for, and the important information which might result from, simultaneous observations on the state of the weather, the direction and force of winds, &c.; if such observations could be taken at places *not too far apart*, and if possible at two or three times a day, if only for a month or two; and further, that *all* the resulting observations should be exhibited on a map for each hour of observation on each day, or on a map for each day only. In the former case I proposed that the different hours of observation should be indicated by a colour, such as red for the morning, blue for midday, and yellow for the evening observations. My object was to obtain *pictures* of the weather, so as to give a view of the changes as they occurred from day to day, as being far more useful than an accumulation of observations exhibited in numerals, which afford but a confused idea of the changing phenomena.

361. In support of my proposition I exhibited maps which gave at one view the direction of all the winds, and the state of the weather as observed daily at various places in England, at 7 or 9 A.M., according to the reports in the "Daily News" for each day in the month of October. My plan for making the maps was this:—I had one of Bradshaw's railway maps pasted on a piece of thin zinc, cut it round the edges to give the outline of the coast, and made a small hole where each place of observation was situate. Laying this map upon a piece of paper, drawing a brush with a little colour round the edges, and dabbing it through the holes, gave me at once a skeleton map: on this the direction of the wind at each place was shown by a small line or arrow; the force of the wind was indicated by the arrow being barbed on one side for a slight wind, both sides for a moderate wind, a line across the tail of the arrow showed a breeze, two lines a high wind, and so on; the fall of rain being indicated by one or more dots, according as the fall was light or heavy; and other marks were given to denote thunderstorms, &c.

362. These maps were not very troublesome to make, and they were very interesting; especially in showing the extremely local character of the winds, as on every day they were blowing from many, on some days from almost every point of the compass; and, at times, at places not twenty miles apart, the wind was blowing in opposite directions.

363. Several remarkable proofs were afforded by these maps of the effects of rain in producing winds, as on several occasions when heavy rains were indicated, there was a marked tendency of the winds at the neighbouring stations towards the place where the rain was falling.

364. If a proper selection of stations were made, and observations taken as I suggest, any phenomena might be indicated by a sign or a colour without confusing the map; and I believe more information might thus be obtained in a few months, than from years of observations registered in the usual manner.

365. Returning to the theory of cycloid storms. From the fact that the winds in England have at most times such a local character, and are so variable in their direction, I am led to believe that similar circumstances must prevail more or less in this hemisphere, in all parts north of the trade winds. If so, the fact that in places at a considerable distance the one from the other, storms prevail at the same time, cannot be taken as a proof of their being parts of one and the same storm: and the proof that they were so ought to rest on the most conclusive evidence.

366. Believing the comparison between the thunderstorm of temperate and the hurricane of tropical regions to hold good both as regards their general phenomena, and *the cause of their progress from place to place,* I will endeavour to explain my ideas as to what this cause may be. I believe the progress of a storm is in many respects analogous to that of a fire. Thus, if a fire be lighted in a field of gorse or underwood, it would make its chief progress in the direction in which the wind may blow; but if it were quite calm, then it would make its way chiefly in that direction in which the greatest amount of fuel might be

presented for its consumption. Now a thundercloud, as preliminary to a storm, can only form during a calm, and at a time when the air is highly charged with vapour; and on the commencement of the storm a vacuum or rarefied space would be produced on all sides of the cloud, from the escape of electricity from the vapour around it; this rarefied space would be greatest on that side of the cloud on which there may be the greatest amount of vapour, and toward this rarefied space the cloud would be propelled more than in any other direction; this would produce a further rarefaction, from the discharge of electricity from more vapour, and a further progress of the cloud in that direction; and thus the cloud would be propelled on and on in a course dependent on the rarefaction produced by itself in discharging the electricity from the vapour in its course, and consequently in that direction in which the greatest amount of vapour would be presented to its influence.

367. If a thundercloud forms in ever such calm weather, the storm will pass off in some direction; thunderstorms often take a course contrary to the wind; two such storms will sometimes approach each other from different directions, and unite; and at times a thunderstorm will divide into two or more portions, each passing off on a different course. It is well known that in some places thunderclouds almost invariably take certain directions, often following the course of a river; and sometimes the nature of the soil, or the direction of hills, seems to affect the course they take. These effects are generally attributed to an electric attraction between the clouds and the earth; and this may be the case in some degree, but I believe the chief cause is the one I assign in the preceding paragraph.

368. All hurricanes which commence in the West Indies pass off across the Gulf of Mexico or, as is more frequently the case, take towards the Gulf stream, and follow its course; and in fact this vast stream of warm water seems to have an influence on all the storms of the Atlantic. The following extract on the subject is from *Maury's*

Physical Geography of the Sea. "I am not prepared to maintain that the Gulf Stream is really the 'Storm King' of the Atlantic, which has the power to control the march of every gale that is raised there; but the course of many gales has been traced from the place of their origin directly to the Gulf Stream. Gales that take their rise on the coast of Africa, and even as far down on that side as the parallel of 10° or 15° north latitude, have, as it has been shown by an examination of log books, made straight for the Gulf Stream: joining it, they have been known to turn about, and, travelling with the stream, to recross the Atlantic, and so reach the shores of Europe. In this way the tracks of storms have been traced out and followed for a week or ten days. Their path is marked by wreck and disaster. At the meeting of the British Association in 1854, Mr. Redfield mentioned one which he had traced out, and in which no less than seventy odd vessels had been wrecked, dismasted, or damaged. A plate, prepared by Lieut. B. S. Porter, from data furnished by the log books at the Observatory, represents one of these storms. It commenced more than a thousand miles from the Gulf Stream, made a straight course for it, and travelled with it many days. There are many other instances of similar gales. Now what should attract these terrific storms to the Gulf Stream?"

369. I will endeavour to answer this question:—From the heat of the waters of the Gulf Stream, a calm for a short time would produce a great accumulation of vapour in the air above it; and in a calm of more than ordinary duration, this accumulation would far exceed its ordinary bounds; and although I presume that one effect of the trade winds must be the carrying away of vapour, and consequently the prevention of any great accumulation wherever they prevail; still I believe that on extraordinary occasions the vapour from the Gulf Stream may overspread, in a general way, the whole of the West Indian islands, or even, at times, overtop the trade winds, and spread across the Atlantic to the coast of Africa. Now if under such circumstances a thunderstorm should

commence in any low latitude to which the Gulf vapour may extend, and an electric communication be thus formed between the earth and the mass of vapour above it, then I believe all the conditions for a hurricane would exist, as all the elements of the thunderstorm would be present, but, in degrees, magnified a thousand times. From the escape of electricity from the vast accumulation of vapour above, the rarefaction would be enormous; the wind, from the rush of air into the rarefied space, would be tremendous; the fall of rain a deluge; and the lightning more or less terrific from causes already assigned (211). And as the great mass of vapour would be in the direction of the Gulf Stream, the storm, although spreading out laterally, would leave the dryer regions of the trade winds, and pass off in that direction.

370. I am not aware of any phenomenon connected with Gulf storms but what may be fairly explained in accordance with the foregoing theory, except the alleged whirling character of them, the reality of which I doubt; although I believe whirlwinds or any other currents may occasionally occur during such violent commotions. As the storm would be as it were dragged along by the constant rarefaction of the air, in the regions of the clouds, produced by the storm itself in the direction of its course, the cause of the sinking of the barometer on the approach of a storm may be readily seen, as this rarefied space would in some measure overrun the advance of the storm below. As the air from surrounding districts would rush into the rarefied space produced by the storm, the most violent winds would be about the edges or exterior to this space, where from the wild commotion the winds may take any and every direction: but about the centre of the storm it might be comparatively calm, for although the air might be rarefied and the barometer very low, the rush of air to restore the equilibrium would only produce a wind at a distance and overhead, when rushing inward and upward into the rarefied space above. Thus towards the exterior limits of a storm the most violent winds may occur, with tremendous rain, or with little or none; and

towards the centre of the storm there may be little or no wind, although the rain may fall in torrents.

371. The hurricanes of the West Indies do not always pass off to the Gulf Stream, but the exceptions to that rule support the views I advance. The course of the hurricane of August 11, 1831, followed the line of the West India islands, and across the Gulf of Mexico, taking just that line where I presume the influence of the trade winds is most intercepted by those islands and the Florida peninsula, and consequently a line of a more or less accumulated vapour. "It commenced near Barbadoes on the night of the 10th, on the 11th it passed over the islands of St. Vincent and St. Lucia, extending a portion of its influence to Martinique and the neighbouring islands on the north, and to Grenada on the south; but exhibiting its principal violence between 12° 30′ and 14° 30′ of north latitude. On the 12th it arrived on the southern coast of Porto Rico. From the 12th to the 13th it swept over the island of Hayti, or St. Domingo, and extended its influence as far southward as Jamaica. On the 13th it raged on the eastern portion of Cuba, sweeping in its course over large districts, if not the whole of that extensive island. On the 14th it was at Havannah. On the 15th the gale was encountered off the Tortugas. On the 16th and 17th it arrived on the northern shores of the Gulf of Mexico, in about the 30th degree of north latitude; raging simultaneously at Pensacola, Mobile, and New Orleans. From the coast of the Gulf of Mexico the storm entered upon the territories of the adjoining states, *where it appears to have spent itself in heavy rains.*"

372. I beg again to remind the reader that all I advance on this subject has been written on the spur of the moment. I have warmed with my subject, and endeavoured to account for a phenomenon of which I know but little, and to which I have hardly given a careful thought till during the last few days; still, on reviewing what I have written, I think I have fairly answered Lieut. Maury's question; and on the theory I offer, it is not difficult to see why the gales of the Atlantic should be controlled

in their march by the "Storm King," or why a storm arising on the coast of Africa should cross the Atlantic to the Gulf Stream, and then carve for itself a path through the dense vapour over the whole length of that body of water. But I beg again to reiterate the opinion that storms *very* seldom run such an unbroken course; and that very erroneous conclusions may have been deduced from a number of storms having been mistaken for one and the same. I have shown that there have been simultaneous tornadoes in America (282); storms in Great Britain (323); and hailstorms in India (359); and I see no reason why similar atmospheric disturbances may not prevail above the Gulf Stream, and simultaneous storms occur in that generally stormy locality. Such storms may be one so far as regards their being produced by one great atmospheric disturbance, commencing in low latitudes, and thence passing to and along the course of the Gulf Stream; that is in fact a hurricane, producing a general stormy condition around it, or a combination of storms, as in England a single thunderstorm will seemingly change the general condition of the weather, and induce storms in all parts of the country.

373. A correct knowledge of the nature of storms, and of the Gulf storms especially, is of the highest importance, and to no country more so than to our own: the theory of their cyclone character has made great progress in public estimation, and many adopt it; it may therefore be considered presumptuous in me to offer an adverse opinion, all I advance being mere theory. Still I believe that no satisfactory knowledge has been, or ever can be obtained on the subject by individual and uncombined observations, however assiduously and carefully they may be conducted. I therefore respectfully submit, that a very considerable addition to the knowledge of the nature of storms would result from a meteorological survey of the Atlantic, on a plan similar to that I proposed for England in 1848 (360). I believe the government of every country, and the seamen of every nation, would readily assist in promoting an object with such strong claims on

the sympathies of all, both on the score of humanity, and the welfare of every one connected with the sea. Printed forms of a simple character, for filling up, might be issued as far as possible to *all* vessels on the Atlantic, the Carribbean sea, and the Gulf Stream. Observations should be simultaneously taken, at such times and as often on each day as may be deemed necessary, and previously arranged. Such observations might be collected; and the phenomena of any hurricane which had occurred could be shown on maps, and depicted by showing *every* observation which could have been in the least affected by the storm, or was taken in the neighbourhood of where it occurred; and this for as many days both before and after the storm as may be deemed useful. By such means it is possible that storms might be traced from their commencement to their termination, and more knowledge on the subject be obtained in a few months than could ever be obtained by other means.

Concluding Remarks.

374. I have now shown how far the proposed theory is applicable to (I believe) every important point connected with rain and its allied phenomena. I am well aware that theorists are too often blind to defects in their own views; it may be so in my case: yet looking at the subject how I will, I cannot see that I have slurred over any question, or unfairly stretched the theory to meet any case, but believe it affords that plain and easy explanation of causes which is most in accordance with the beautiful simplicity of nature's operations. Can as much be said of any other published theory on the subject? I believe that no one can take the various phenomena, one by one, as I have done, and attempt to explain them as readily by any other theory, without utterly failing before they have taken many steps. If I am right in this opinion, then I may fairly ask that the theory I submit should receive at least a like attention to that which has been bestowed on other theories on the subject.

375. The principal objection which I believe can be

advanced against the theory is, that I cannot prove it. There is much in this, as in all theories, which will not admit of positive proof; but still much may be provable. I could not prove the electricity of steam, although I was mentally assured of the fact, and tried a few insufficient experiments to prove that steam would give off electricity on being condensed, and this long before the fact became known by accidental discovery. If it be objected that I ought to have proved more in support of the theory, I beg again to revert to the circumstances under which I have investigated the subject in question. From the early period of ten years of age, I have had but little real leisure time, and very little money to expend in experiments; I have had to fight my way upwards in life by the work of my hands, and have had some difficulties to encounter from which many are spared; the time I have given to scientific pursuits has, in a great measure, been at a pecuniary loss, or at a sacrifice of recreations such as most enjoy. And I may add, that the hours occupied in writing this Essay are from those usually devoted to rest and sleep.

376. It is under these circumstances I now beg that the theory should receive more attention than has hitherto been bestowed on it, and that its merits be fairly tested; this I believe I may ask with propriety. If it be found worthy of further investigation, or can be established, let it take its place as a theory in accordance with its merits; or if it can be shown to be erroneous, then let it be set aside as worthless. In the latter case I may feel disappointed; in the former I shall have the satisfaction of knowing that I have not devoted the leisure, or more than the leisure, of so many years in the pursuit of a mere fallacy.

377. Many experiments might be applied to test the theory, but I believe none would be so efficacious as that for producing rain by raising electric conductors to the regions of the clouds by the aid of captive balloons (162). It is now twenty years since I first proposed this experiment; and if I have shown the probability of its

success, I trust that means may be adopted for trying it. Hundreds, aye, thousands of pounds are expended every year on experiments of far less importance, and by no means so interesting. I therefore hope that the government, or some of our scientific societies, will have the experiment fairly tried; or if not, I believe it might be done by private subscriptions. But be it as it may, my occupations will not permit me to take much part respecting it, so I leave the matter to the public care.

378. As to the importance of the experiment, I propose it simply as a test to the theory, and (as stated in my pamphlet in 1841) "it would be useless for me to give an opinion whether this formation of rain would ever be of any real utility: but I believe if it could, there is no country to which it would be of more value than to Great Britain, as her colonies are distributed in all parts of the world, and to some it may be of the first importance, as in many districts the fall of rain has been diminished by cutting down the forests on hills, and other means." However, the great importance of the experiment would be in the knowledge it might afford as to the real nature of clouds, and the cause of their suspension. Many experiments have been made in France for the prevention of such dreadful hailstorms as sometimes occur there; a better knowledge of the nature of clouds might lead to important results on that head, and if so, it would be a result for all to rejoice in, if on the score of humanity alone.

379. In the prospectus I issued for this publication, I gave a list of papers I had written on meteorological and magnetic subjects; these I have appended, thinking that some may wish to refer to them. I also intimated my intention to publish a similar work on the Aurora and Terrestrial Magnetism; the publication of that will in some measure depend on the reception of this Essay. I shall gladly receive the names of any persons willing to subscribe for its publication on the same terms as for this; but I cannot promise that it shall be published, as the investigation necessary for my purpose may require more

time than I may be enabled to devote to the purpose. But the papers above alluded to will give a fair idea of my views on the subject, which are, that the phenomena of the aurora, and the directive properties of the magnet, are produced in a great measure by the causes productive of rain. If this opinion be correct, it affords another proof of the simplicity of the works of nature; for as in chemistry we find that a few gases combined in various proportions are productive of an almost endless variety of substances, so also I believe that the simple properties of electricity, which I take as the foundation of the proposed theory, are productive of an almost infinite variety of effects.

380. I have also appended a few observations on lightning and its effects, together with some extracts from Beccaria's work on the electricity of serene weather.

381. On reviewing what I have written, I fear I have laid myself open to the charge of presumption; if so, I beg that some allowance may be made for the enthusiasm which has induced me to devote so many years to the study of these subjects. There are repetitions which may be tedious, and doubtless mistakes. On this head, I again plead the circumstances under which the Essay has been written, as in addition to what I have already stated, I may add, that I have had my ordinary occupation to attend to, and the transition from paper-hanging to writing this paper have often been abrupt, and the two subjects almost blended together.

382. In conclusion I beg to tender my sincere thanks to all who by subscribing have aided me in the publication of the Essay, and trust that if I have failed in other respects, I have succeeded in showing to the most unscientific reader the vast field there is for mental recreation and reflection even in the formation and fall of a drop of rain.

APPENDIX.

On some Appearances and Effects of Lightning.

383. AS I have given so much attention to the phenomena of thunderstorms, it may be expected that I have seen many unusual appearances in lightning, &c.; but if some published accounts are correct, I have been very unfortunate in this respect. I believe that from the sudden and startling effects of lightning, observers too often give way to their surprise, and sometimes to their imagination. No phenomenon is more likely to deceive an observer than that of lightning, and I believe, that on this head, I cannot do better than quote from a paper by Professor Faraday, "*On some Supposed Forms of Lightning:*"—

384. "The magnificent display of lightning which we had on the evening of the 27th of last month, and its peculiar appearance to crowds of observers at London, with the consequent impression on their minds, induces me to say a few words on certain supposed appearances and forms of lightning, respecting which the judgment of even good observers is often in error.

385. "When after a serene sky, or one that is not overcast, thunder-clouds form in the distance, the observer sees the clouds and the illumination of the lightning displayed before him as a magnificent picture; and what he often takes to be the forked lightning (i.e. the actual flash, and not the reflection of it), appears to run through the clouds in the most beautiful manner. This was the case on that evening to those who, being in London, observed the storm in the west, about nine o'clock, when the clouds were at the distance of twenty miles or more;

and I have frequently observed the same effects from our southern coast over the sea. In many of these cases, that which is thought to be the electrical discharge is only the illuminated edge of a cloud, beyond and behind which the real discharge occurs. It is in its nature like the bright enlightened edge which a dark well-defined cloud often presents when between the sun and the observer. In the case of its production by lightning and distant clouds, the line is so bright by comparison with the previous state of the cloud and sky, so sudden and so brief in its existence, so perfectly defined, and of such a form, as to lead every one at the first moment to think it is the lightning itself which appears.

386. "But the forms which this line assumes, being dependent on the forms of the clouds, vary much, and have led to many mistakes about the shape of the lightning flash. Often, when the lightning is supposed to be seen darting from one cloud to another, it is only the illuminated edge which the observer sees. On other occasions, when he was sure he saw it ascend, it was simply this line more brilliant in its upper than its lower part. Some writers have described curved flashes of lightning, the electric fluid having parted from the clouds, gone obliquely downward to the sea, and then turned upwards to the clouds again: this effect I have occasionally seen, and have always found it to be merely the illuminated edge of the cloud.

387. "These forms frequently appear to be in the cloud, and yet are not distinguishable till the lightning occurs. It is easy, however, to understand why they are then only developed, for that which appears in the distance to be one dull mass of cloud, distinguishable in figure only in its principal outline, often consists of many subordinate and well shaped masses, which, when the lightning occurs amongst or beyond them, present forms and lines before unperceived."—*Philosophical Magazine, August*, 1841.

388. As far as my own experience goes, I have never observed any appearance of lightning but what I believe

may be considered as being either the direct passage of an electric spark, or the reflection from one. I have seldom seen any great display of summer or sheet lightning, either when there were only very light clouds, or when the sky was perfectly cloudless; but I have generally found that, at the time, a heavy storm occurred in the direction in which the flashes appeared, although sometimes at a distance of sixty or seventy miles. And it is a well ascertained fact, that flashes of light have been seen at a distance of one hundred to one hundred and fifty miles from the place where the lightning actually occurred; while we have no authority for believing that thunder is ever heard at a greater distance than fifteen miles, and even under very favourable circumstances, seldom so far as ten miles off. This is a very curious fact, as the firing of cannon has been heard at a distance of eighty or ninety miles.

389. It has been asserted that lightning sometimes strikes from the earth to the clouds. I have never seen any such phenomenon, although I have watched for it earnestly: and from all I have seen, and considering the question theoretically, I cannot conceive the possibility of such an occurrence; for considering the earth as the great source of electricity, and the electric condition of the earth its normal state, I can well conceive that the excess of the electricity of a cloud would accumulate on the surface of the cloud, so as to overcome the resistance of the air and fly off to the earth; but I cannot understand how a mere aggregation of very minute particles of vapour, floating in the air, could exert such an attractive force as to disturb the electric condition of the earth sufficiently to attract lightning from it. And in fact, *if the theory I advance be correct,* clouds can only exist in an electrically surcharged condition with respect to the earth, and if so, it is impossible that lightning could pass from the earth to the clouds. Vessels have often been struck by lightning, but I never heard of a ship at sea being affected in any way by an upward stroke of lightning.

390. I have seen lightning of a peculiar form, and I believe that some such appearances may have led observers

to think that lightning has passed upwards from the earth, as when striking along the clouds from a distance towards the zenith, it *seems to* rise from the earth to the clouds. The few occasions when I have observed this form of lightning have been by night, after a *heavy* thun-storm has passed off to a distance of four or five miles, and a thinnish stratum of clouds has overspread the other part of the heavens; when it seemed as if the electricity, striking from the upper to the lower clouds, divided into streams or branches in distributing itself through the more negatively electrified stratum of clouds.

391. The direction in which the lightning passed has been quite unmistakable, as it started from the direction of the thunderstorm along the lower clouds; for although the whole flash was pictured to the eye at once, still it was not with the *instantaneous* effect of ordinary lightning, but there was an appreciable time as it branched out into its various ramifications.

392. These streams or branches were not straight, as if darting from point to point, but ran in crinkled lines. I believe they could not well be regarded as direct flashes of lightning throughout their whole course, but as a series of discharges from cloud to cloud as the electricity dispersed amongst them. The streams of light certainly were not merely the illuminated edges of clouds, as I have seen them pass near the zenith. No thunder seemed to proceed from this lightning, although there has generally been thunder directly after it passed, but apparently from the thunder-cloud at a distance. And, as far as I remember, little or no rain has ever accompanied it, even when passing nearly overhead.

393. A display of such forms of lightning occurred at Manchester and its neighbourhood on the 16th of July, 1850, which are described in the *Philosophical Magazine* for August of that year by Mr. J. P. Joule, and by Mr. P. Clare in the November number. Illustrations are given by both those gentlemen of forms they noticed, and figures 1, 3, and 4 in Mr. Clare's so exactly resemble what I have seen, that I have taken the liberty of giving

a copy of them in fig. 3, 4, and 5 in the plate with this essay.

394. Lightning is described as sometimes taking the form of globes or balls of fire; and I believe it is probable that every stroke of lightning might have this appearance if passing very near to an observer, to whom it could only be visible through a very short part of its course; i. e. that it would appear as a globe or large spark, and not as a streak of lightning.

395. I was one night (some years since) standing at the south-west corner of the Parks (near the site of the University Museum), watching a thunderstorm which was at a distance in the north-east, and my attention was so taken with it that I did not notice the state of the weather overhead; there was a little rain falling, and I was leaning against a doorway, partly for shelter and partly for rest; when, with no previous increase of rain, or premonitory indications, the lightning struck to the earth only a few yards before me, and its appearance was so different from that of lightning when seen at a distance, that it might have been described as a ball of intensely bright molten metal, dashed upon or bursting on the ground; as it seemed to fly into hundreds of pieces, or rather it seemed to be a globe and in pieces at the same time. As for myself, I heard but one report, and that as if a cannon had been fired off close to my ears; and in fact I was, as it were, at the same instant, stunned by the thunder, blinded by the lightning, drenched with the rain which seemed to dash down with it, and completely cured, for a week or two, of my propensity for watching thunderstorms.

396. On enquiry, I found that the thunder, which to me was a single report, was in fact one of those very sharp, rattling claps of thunder resembling the firing of artillery in rapid succession; and the lightning, which seemed to me almost globular, was seen to strike down in the usual stream-like manner. No marks were left the next morning where the lightning had struck; nor could I expect any, as the ground had been recently ploughed, and the

rain had been very heavy. The apparent bursting of the lightning on the ground, I attributed to the instantaneous dispersion of the electricity as it reached the earth. I did not fall from the effects of the lightning, as I was leaning against the door at the time, otherwise I certainly should have done so, as I seemed to be electrified at the instant, although not struck by the lightning; and thus I believe persons are often affected, and fall, not from being actually struck by lightning, as they imagine, but owing to the limbs being acted on by the electricity in its dispersion; i. e. their being electrified for the moment.

397. As to the effects of lightning, I have seen nothing worthy of remark except in two cases, both of which seem of some importance as regards the position and use of lightning conductors.

398. The first case was that of St. Ebbe's church, in Oxford, being struck by lightning on the 14th of July, 1855. The lightning stroke fell on the vane and roof on the tower, from whence the electricity passed off down and along a tinned-iron water pipe, of two inches and a half in diameter, for about ten feet to its termination; thence, taking the course of the water discharged from it, down the slated roof of the church, to a tinned spout under the eaves. This spout was about eighteen feet from the ground, and from it two pipes descended, *both of the same diameter, and of similar materials to that of the pipe from the top of the tower to the slates.* One of the pipes (that nearest the tower) reached from the spout to within four feet of the ground, and it was obvious that some of the electricity had passed off from it, as there was evidence of its effects along the spout and down the whole length of the pipe. *The other pipe* was forty feet further from the tower, and reached from the spout to within an inch of the ground; it had been raining heavily for some time before the church was struck, and there was no drain to carry the water away, so that the bottom of the pipe must have been immersed in water, and therefore all tended to promote the ready dispersion of electricity from this pipe. There were marks of electric effects along the

spout to the top of this pipe, and down it to within seven feet of the ground, where the electricity left it and passed through the stone wall, disjointing the stones, scattering the dust and morter about the church, taking off the hinge of the pulpit door, and thence to a *small* gas pipe, where all traces of its effects ceased. The wall was about two feet three inches thick, and as dry as stone walls usually are; the hinge of the pulpit door was nearly in contact with the wall, but the gas-pipe was nine inches from the hinge, and not more than three-fourths of an inch in diameter.

399. The passing off of the electricity from the outer pipe could not have been from its being insufficient in size, as the pipe on the tower, although no larger than each of the lower ones, was sufficient to conduct the whole discharge from the top of the tower to the slated roof below; the electric force must also have diminished during its course, owing to the heavy rain making every surface exposed to it in some degree a conductor. It was also evident there had been an electric discharge down the shorter pipe; and yet there was the fact, that a small pipe had attracted the electricity from the larger one, although at a distance of upwards of three feet, and separated from it, with the exception of the hinge, by very bad conductors.

400. It seemed that the attraction of the gas-pipe was not merely in proportion to its size, but was increased by and consequent on its connection with the whole gas-pipe system of Oxford. The case therefore is important as regards the erection of lightning conductors, as it shows the necessity for avoiding the proximity of gas-pipes and the like, wherever a discharge from a conductor to them might be injurious; and also that in some cases lightning conductors would be increased in efficiency if made to terminate in contact with any large amount of metal, distributed over a large space, such as the water pipes or gas pipes of a town, or perhaps the metal of a railway.

401. The other case to which I allude occurred on the night of the 17th of June, 1858, when a farm house, near

the Woodstock-road station on the Great Western railway, was struck by lightning. The house is of two rooms in height, and has four rooms on a floor, two in front and two behind them; the roof being double pitched, so that a large lead gutter runs the length of the house. In a line with the front of the house, and attached to it, are the stables, but they being only half the depth of the house, the metal spout under the eaves at the back of the stables is in a line and connected with the lead gutter on the house.

402. The lightning stroke shattered and broke off a tree standing at the corner of the house, but about two yards from it, and entered six of the eight rooms. In the stable a horse was killed, while six others standing side by side with it, five on the one side and one on the other, were uninjured. Behind the horse which was killed, and almost in contact with its hind quarters, was an unglazed window, open to the farm yard; and another horse was killed in the yard, which must at the time have had its head close to this window, as hair was found sticking to the frame where the horse fell against it when struck. Providentially none of the family were injured.

403. It was not difficult to account for the effects on the house; the lightning seeming to have been conducted into the upper front room by the iron brackets, supporting a wooden spout, to the iron bars of the window, which it descended, doing but little mischief, but shattering the window frame, &c. in the lower room where there were no metal bars. The electricity entered the other part of the house from the lead gutter on the roof; at the one end passing into a cupboard directly under the gutter, and against the door of which parts of a bedstead were standing, and amongst them an iron rod; this formed a conductor to the floor where the electricity entered the wall, breaking out again in the room (or beer cellar) below, just above a large fryingpan hanging on the wall, which, with other things, conducted it to the ground. At the other end of the house it also entered a cupboard under the lead gutter, where it pierced the band-box before alluded to

(67); in this case the electricity seemed to have been attracted by the mass of metal below, connected with the copper, the grate, &c. as usual in a farm house kitchen.

404. I was at first puzzled to account for the horse being killed in the stable, which, after the preceding unusually hot weather, was very dry; while, from the very heavy rain falling at the time, everything without was soaking with water. There were no marks of electric effects in the stable; however, at the back, just beneath the spout, were several ventilation holes in the wall, and I thought that the warm air from the stable, together with the breath of the horses, might have formed a sort of conducting medium for the electricity from the spout: I was therefore led to examine the holes, and then could not mistake the cause of the accident; for in the hole, almost directly over the place where the horse was killed, was an old ploughshare, just under which a large iron wheel was hanging on the wall; this reached the rack, which was filled with green vetches at the time of the accident, and from which the horse was taking some at the moment, as its mouth was full when it was found dead. Thus there was as curious a combination of conductors as could well be imagined, as the electricity seemed to have passed from the lead gutter on the house, along the spout to the ploughshare, down the iron wheel, and through the vetches to the horse, which, from the dry state of the ground on which it was standing, was in a great degree insulated, and consequently formed a conductor for the electricity to the horse in the yard, as the two horses must have been nearly in contact, and between them a quantity of harness and iron chains were hanging.

405. That this view of the phenomenon is correct I have not the slightest doubt; and I give the case, not only as being curious, but also as a striking instance of electricity finding for itself a passage by the best conductors within its reach, and showing by what trifling circumstances its course of distribution may be determined. It also shows the importance of attention to the arrange-

ment of waterspouts, pipes, &c.; for it is evident that, had there been a pipe of any kind of metal to carry off the water from the lead gutter, and consequently connecting it with the ground, the electricity would have been conducted away by it, and not have entered either of the back rooms in the house or destroyed the horses; as in the courses the electricity passed off by, there were no continuous conductors.

406. Buildings are often protected from lightning by the lead or other metal roofs, gutters, pipes, &c. forming a system of conductors, and this often from accident rather than design; and a few years ago I had a singular proof of the necessity for attention to such matters. I was requested by the proprietor of a house, newly erected in an exposed situation, to give my opinion as to its safety from lightning. I had previously noticed it in this respect, and thought that no place could be more effectually protected, as the flat part of the roof was covered with lead, which was continued down the ridges and angles to the lead gutter within the parapet; and from the level of this gutter several metal pipes descended to the ground, making, apparently, a complete system of conductors. But when I looked more closely into the matter, I found that the metallic connection of the gutter with the pipes had been cut off by a length of terra cotta pipe in all the cases forming the connection between them.

407. In alluding to the question of conductors, I am rather departing from the real subject of the essay, but I do so, thinking it may not be altogether useless; as the importance of conductors is often overlooked, and I believe large masses of metal, such as iron girders, &c. are often inserted in buildings without any precaution in guarding against the effects of a lightning stroke.

Extracts from an Essay on the Mild and Slow Electricity which prevails in the Atmosphere during Serene Weather. Translated from the original Italian of Father Giambatista Beccaria. London, 1776.

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408. "996. With respect to the electricity of the atmosphere during *serene weather*, it is to be investigated only by using exploring wires of metal, that are very long, and especially placed very high; for as such electricity is commonly very weak, it would, by operating otherwise, often prove a difficult matter to ascertain its existence, and much more so its nature."

409. "997. For that same reason, viz. the weakness of the electricity to be examined, I do not recommend sharp points; they would too readily draw off and dissipate such electricity."

410. "998. These previous essential cautions being laid down, I think I cannot better continue to indicate the manner in which a pretty good apparatus may be settled, than by shortly describing that with which I am observing at this instant."

411. "999. In the first place, with regard both to the *openness* and height of its situation, it is settled on the pleasant hill of *Garzegna,* which is situate in the neighbourhood of Mondovi; and from which the whole compass of the Alps, as well as the whole plain of Piedmont, is easily discovered. This vast extent both of country and sky, that opens before the above hill, I take particular notice of, as contributing much to the success and certainty of the experiments." He then describes his apparatus, consisting of a long insulated iron wire, elevated from the top of the building, on lofty poles, trees, &c.

412. "1007. *Ever since I began to observe the atmo-*

spheric electricity during serene weather, the whole series of my observations has confirmed it to me, that this electricity is constantly of the excessive, or positive kind."

413. "1008. On the mountain of St. Michael, I indeed happened, three times in fifteen days, to find, during serene weather, the wires electrified by *deficiency*. But then I took notice that the high mountains over the town of Susa, from which an impetuous squally wind was then blowing, were surrounded by clouds, the inside of which was much agitated, and their top was lengthened like rising smoke, towards the place in which I observed."

414. "1010. It was fifteen years after the abovementioned observation, when I happened to meet with another instance of such inversion; on the 13th of August, 1771, during serene weather. This circumstance we wrote, in the account we kept of our common observations, to have taken place 'during the time a very strong wind blew from the mountains, which towards north-west hide the mountains of Lanzo, behind which we saw clouds rise, shaped like exhalations of smoke;' in short, similar to those just now described." This observation was made on the top of the mountain of Superga.

415. "1012. No other instance of defective electricity, in clear weather, besides the above, I have succeeded to perceive, during three years I have since continued to observe on the mountain of Superga. Neither have I happened, in my observations in the Valentin, or in those which I have continued to make here in Garzegna, for several months every year, or in those I have occasionally made in Cigliero, Andrà, Alba, and other places, to meet with any instances of the like electricity, during clear weather. Only on the 18th of April of the present year (1775), since I am again come back here to Garzegna, I have met with another instance of the same kind." Here, after describing the particulars, he continues, "In fact, the Apennine mountains are surrounded by clouds, the upper edges of which are divided into numbers of rectilinear spouts, like rising smoke, the tops of which are bent towards us; the clouds under these clouds look black, as in a storm."

416. "1014. The above instances of defective electricity during clear weather, sufficiently indicate how far they were exceptions to the general rule, that an excessive electricity constantly obtains in such weather, and showed that such electricity was rather brought over, by means of the wind, from some part of the sky which was at that time either cloudy, or snowy, or rainy."

417. "1015. The above instances, which indeed are very rare, of electricity being thus brought over from a very great distance, does not, after all, materially differ from those frequent instances in which, though the sky just over our heads may be clear, dark thick clouds, which draw continually nearer to the place of the observation, send an electricity which is found to be alternately excessive and deficient."

418. "1016. Nor do I think, however, that the six times I have happened to meet with an instance of defective electricity during clear weather, are to be considered as too small a number to draw any conclusion from. If it be considered that those instances, or exceptions to the constant electric state of the weather in clear weather, are the only ones I have met with during a great number of years, and that every one of them has been accompanied by the same circumstances of distant dark clouds, resembling exhalations of smoke, it will be thought that those six exceptions, taken together, are a sufficient number for ascertaining both their *unity*, and the manner in which they were effected."

419. "1019. *I find that the electricity during clear weather is constantly connected with the state of the air, as to moisture and dryness.*"

420. In the following paragraph we have good grounds for estimating the assiduity of this very accurate observer, as after giving many directions as to the many precautions necessary in observing, he says:—

421. "1034. To the above extensive and nice considerations, great assiduity in observing must be added. Thus, to speak of myself on this occasion, whenever I have sufficient leisure to repair hither, to Garzegna, where besides

other conveniences, I meet with the advantage of solitude, I spend my whole time in observing. I live night and day in the high and open room I have chosen; there the *deferent* (indicative) wire brings me incessant information of the state of the external electricity, and subjects it to a very sensible electrometer. From thence surveying all parts of the horizon around me, at every instant I have an opportunity of comparing the varying state of this electricity, with that of the weather or sky."

422. "1036. Proposition VI. *The moisture in the air is the constant conductor of the atmospheric electricity during clear weather; and the quantity of such electricity is proportioned to that quantity of the above moisture which surrounds the exploring wire; except such moisture also lessens the exactness of the insulation both of this wire and of the atmosphere.*"

423. "1037. I do not mean in this proposition to point out the principle itself which produces the electricity in question, but only to ascertain that medium in which it is inherent, and to the quantity to which it is generally proportioned."

424. "1042. In making the above observations on the electricity of the atmosphere during clear weather, I soon perceived that it was equally essential to make account of the frequency of this electricity, that is, of the velocity with which it arose again after being annihilated, as of its intensity itself. In fact, the quantity of the electric fire which accumulates in the exploring wire, continually varies, according to circumstances; and this quantity is not less constantly proportioned to the divergence produced by the electricity in question, than to the frequency of it. This frequency I have commonly computed from the number of seconds which elapsed before the balls again manifested any sign."

425. "1043. In the first place, I have constantly observed that, whenever the electricity obtained, while the air was damp, whether such dampness arose from the heat of the air, which enabled it to take in a greater quantity of moisture, or its density, or whether it was accumulated

only in the vicinity of the exploring wire, this electricity was more frequent, than when the air was dry to any great degree."

426. "1044. In fact, while such a dry state of the atmosphere as above obtains, more than a minute is sometimes requisite to have the electrometer again manifest some sign of electricity; whereas in the damp state of the air I mention (especially if such dampness proceeds from heat, for then it lessens much less the exactness of the insulation), a second of time has scarce elapsed when very rapid oscillations of the balls again take place between my finger, and the sheet of brass which stands between them."

427. "1045. Such constant observations rendered it evident to me, that in the abovementioned damp state of the weather, a much greater quantity of electricity was exerted around the exploring wire, than in dry weather. But that same great quantity of electricity was but very inconsiderable when compared to that quantity which must needs be dissipated from both the atmosphere and the wire, in consequence of their insulation being lessened by the dampness of the air we suppose."

428. "1046. This conclusion, which I drew by analogy from other experiments, was moreover immediately demonstrated to me by the different accidents I observed in the atmospheric electricity itself during clear weather. I constantly found that, whenever a dampness arose in the air from cold, especially when the vapours gathered and condensated near to the surface of the earth, the frequency of the electricity indeed increased, but its intensity was lessened: now, this decrease of the intensity of the electricity, combined with the increase of its frequency, certainly indicates a very great deperdition of the same."

429. "1047. Whence we are to conclude, that, however true it may be, that the quantity of electric fire which is exerted in the atmosphere during clear weather, is proportioned to the quantity of moisture contained in it, yet, as this very moisture is the conductor of such fire, it

thence follows, that whenever a somewhat considerable and lasting condensation of it takes place, every perceivable sign of electricity must cease in the exploring wire."

430. "1057. Proposition VIII. Low and thick fogs (especially when, in their rising, they find the air above them pretty free from moisture) carry up to the exploring wire, when they reach it, an electricity which becomes manifested by frequent little sparks, and produces a divergence of 20°, 25°, or even 30°. If the fog grows sluggish, and continues round the exploring wire, all electricity soon fails: if it continues to rise, and another cloud succeeds, it brings to the wire a fresh electricity, though less than the former. Sky-rockets sent through such thick, low and continued fogs have often afforded me signs of electricity. However, I never happened, under any one of the circumstances above described, to meet with an instance of defective electricity; except perhaps once, when I sent a sky-rocket (to which, like those above, a long string was fixed) through a low thick fog; though I had afterwards every reason to think that I had mistaken a false little star for a true one."

431. This proposition is supported by a number of observations; in describing one of which, he says:—

432. "1065. On the latter end of March, I climbed the high and steep mountain of St. Michael, and there I stretched and insulated several iron wires;" these are described as being, all together, upwards of 2000 feet in length.

433. "1068. ...... Certainly, among the many effects which aërial electricity may produce, the following is abundantly confirmed by experience, viz. that all the vapours, or moist effluvia whatever, which are anyhow brought to rise in the atmosphere, or which swim, or descend in it, are affected by the aërial electricity, in their absolute as well as relative motions. Thus for instance, several admirable accidents of the dew, or of hoar-frost, the tendency of their drops, or icicles, to certain particular bodies,—this tendency obtaining from all sides, though more particularly directed to the angles, edges, and points

of such bodies, all these are accidents which suppose a perpetual electricity in the atmosphere," &c.

434. "1082. But, to say the truth, the aërial and artificial electricity I raised in the room, was in reality an *aereo-vaporous* electricity; that is to say, it was diffused through the moist effluvia which swam in the air in it; and in like manner, the natural electricity of the atmosphere during clear weather, resides chiefly in the vapours diffused in it."

435. "1112. Proposition XIII. 1, *The friction of winds against the surface of the earth, is not the cause of atmospheric electricity.* 2, *Impetuous winds use to lessen the intensity of the electricity of clear weather.* 3, *If they be damp, they lessen its intensity in proportion to the diminution they cause in the exactness of the insulation, both of the wire and of the atmosphere.*" This proposition is proved by various observations; as the following:—

436. "1113. Ever since the atmospheric electricity was discovered, I have endeavoured, after numbers of different manners, both in very dry, and in damp weather, to raise some electricity in different bodies, by means of currents of air artificially produced. Several times I have tried rapidly to turn four bands of gilt pasteboard, fixed like wings, to an axis insulated by means of sticks of glass. At other times I have for a while kept driving air against sheets of metal, with a pair of bellows, and sometimes, against bands of linen cloth, either very dry or wet, which were stretched within a frame, and insulated with silk strings. I have also tried to set fire to squibs which turned with great velocity around a stick, or peg, exactly insulated: now, neither in this peg, nor in the abovementioned axis, which consisted of an iron rod, nor in the above sheets of metal, nor in the bands of linen cloth, could I ever perceive the least degree of electricity, both when the current of air took place, or after."

437. "1115. *Experiments of this kind are certainly very apt to raise doubts about the opinion which certain closet philosophers so gratuitously propagate, which is, that the*

whole atmospheric electricity is produced by the friction of the air against the surface of the earth."

438. "1116. Such opinion is besides abundantly confuted by the observation which forms the second part of the present proposition, which is, that winds commonly weaken the atmospheric electricity during clear weather."

438. "1117. In three different cases I have found the electricity of clear weather entirely annihilated by the wind. I describe one of those cases in my letters to Sig. Beccari, in which the wind, besides being impetuous, moreover appeared to be extremely dry; but the same has also happened when the wind was evidently damp. I shall observe, by the by, that if the electricity in any degree arose from the friction of winds against the ground, its intensity would be found greatest near the surface of the earth."

439. "1120. I have likewise found impetuous winds to have destroyed the electricity of exploring wires, though the latter were placed very high above ground, and of kites, though I then made them rise to a great height: such impetuous winds arose at the time showery clouds were in sight, which they almost instantaneously dissipated. I find nine such instances in the journal of my observations in the Valentin." Several other paragraphs are given, all showing the like effects of dry winds.

440. 1128. refers to the effects of moist winds. "Even those which seem to be very dry, at last often introduce some moisture. Easterly winds, during these three months, which, when fair weather is settled, use here in Garzegna, to rise pretty strong about noon, at three or four in the afternoon, by wheeling to the south-east, become damp to a very perceivable degree; and in the beginning, that is in April, or the first days of May, though the electricity still continued to be of a pretty slow kind, they often remarkably increased its frequency, so that the balls of the electrometer, which, before this damper wind rose, acquired a fresh electricity only after 20″ or 30″ of time, now re-

covered it after every second." Three following paragraphs show the like results.

" On the Electricity produced by Evening Dews."

441. " 1133. From the beginning I thought that bare reasoning and analogy could demonstrate the existence of the electricity that is produced by dew."

442. " 1136. It was in the following year (1758), that I settled an exploring wire, 1000 feet long, here in Garzegna, with which I several times discovered, especially towards the latter end of October, that an electricity of dew took place, which was of pretty considerable intensity. Ever since that year I have observed and set down, that such electricity took place in clear and dry weather, during which no strong wind prevailed."

443. " 1139. Lastly, this present year, I have again repaired to Garzegna, in order to make new observations (for it is by constant observing alone that I learn how to observe properly), and it seems to me that, with respect to the electricity of evening dew, the following propositions may be laid down as founded on facts."

444. " 1140. Proposition XIV. In cold seasons, if the sky is clear, no strong wind prevails, and a pretty great degree of dryness continues to obtain, an electricity of considerable intensity arises after sunset as soon as the dew begins; the frequency of such electricity is moreover greater than that of the *daily* electricity, and it vanishes with great slowness."

445. " 1141. Proposition XV. In temperate or warm seasons, if the same circumstances as above take place, an electricity, entirely similar to the former, arises as soon as the sun has set; only, its intensity is not so constant, it begins with more quickness, it rises to a greater frequency, and ends sooner."

446. " 1142. Proposition XVI. If under the above circumstances, respectively, the general dryness of the air happens to be less, then the electricity that arises in the evening, when the dew begins, is less, in proportion to the diminutions of the exactness of the insulation of both the

exploring wire and the atmosphere that then take place; but then, correspondently to the greater quantity of dew, the frequency of the electricity is greater."

447. "1143. Proposition XVII. The electricity of dew depends, it seems, on the quantity of dew, as the electricity of rain depends on the quantity of rain; and the peculiar manner after which this dew takes place, influences the electricity, in the same way as does the peculiar manner in which rains takes place." Sixteen paragraphs are given in illustration and support of these propositions, and they conclude as follows:

448. "1159. The consistent variety of the above accidents fully demonstrates the constant connection that obtains between the dew and the atmospheric electricity; for the manner after which dew takes place, varies as regularly as the manner of rain does, according to seasons. Nor is it necessary that the dew should be very copious in order to produce a strong electricity; on the contrary, a very great quantity of it would at last destroy the insulation of both the wire and the atmosphere. The electricity itself of rain, the intensity of which is so superior to the latter, likewise requires that the exploring wires should be in some measure insulated; and in proportion as the rain becomes more extensively diffused, this electricity grows less sensible; and it only recovers its former intensity, when a change takes place in the nature, or rather the manner of the rain. Now dew is constantly diffused in great plenty towards the ortive hemisphere, and its electricity may be compared to that of a very rare and subtle rain; and it therefore requires some degree of insulation should obtain in that part of the atmosphere which is little raised above ground, in which alone we can observe it: dew must therefore be in small quantities, that it may not diffuse away its own electric fire."

449. "1160. The *daily* electricity, to sum the whole in a few words, is therefore, like an electricity of a very rare fog, which rises, grows dilated, and thus lessens continually less and less the insulation: the nocturnal electri-

city is like an electricity of rare and subtle rain, which descends, gathers together, and continually lessens more and more the insulation. Hence the daily electricity is of a continuous kind; the nocturnal frequently fails; and it only attains its greatest degree of intensity, when the increase of that moisture which is the conductor of it, happens to take place without injuring the insulation."

The following papers on Meteorological and Magnetic subjects have been written by the Author:—

1. Conjectures on the Cause of Rain, Storms, the Aurora and Magnetism; with a Suggestion for Causing Rain at will.

   Read at a Meeting of the Ashmolean Society, 1839; at the Meeting of the British Association at Glasgow, 1840; and published in a pamphlet, 1841.

2. On the Connection of Evaporation with Electricity.

   Read at a Meeting of the Ashmolean Society, 1841; and published in the Lond. and Edinb. Phil. Mag. 1842.

3. On the Cause of the Electricity of Steam.

   Read at a Meeting of the Ashmolean Society, and published in the Edinb. New Phil. Journal, 1844.

4. On the Phenomena of Evaporation, the Formation and Suspension of Clouds, &c.

   Published in the Edinb. New Phil. Journal, 1845.

5. On the Cause of Storms, and the Fluctuations of the Barometer.

   Read at a Meeting of the Ashmolean Society, and published in the Edinb. New Phil. Journal, 1846.

6. On the Cause of Terrestrial Magnetism.

   Read at a Meeting of the Ashmolean Society, and published in the Edinb. New Phil. Journal, 1847.

7. On the Aurora, and Declination of the Needle; and,

8. On the Cause of Evaporation, Hailstones, and the Winds of temperate regions.

   Read at the Meeting of the British Association at Oxford, 1847.

9. On the Height of the Aurora-Borealis.

Read at a Meeting of the Ashmolean Society, 1847; and published in the Edinb. New Phil. Journal, 1848.

10. On a Plan for Simultaneous Meteorological Observations.

Read at a Meeting of the Ashmolean Society, 1848.

11. On the Change of Temperature in Europe, and the Variation of the Declination of the Needle.

Read at a Meeting of the Ashmolean Society, and published in the Edinb. New Phil. Journal, 1853.

12. On the Storm of Sept. 22, 1856.

Read at a Meeting of the Ashmolean Society.